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LOCAL COMMUNITIES OF THE BOTHNIAN ARC IN A PREHISTORIC WORLD

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Abstract

The aim of this study is ultimately to expand our understanding of the prehistoric world closer to the individual. The focus is on the Bothnian Arc and its local communities lost in time. To understand them, we need to understand the wider Central Fennoscandian prehistory. The main timeframe is 5500 BCE to 600 CE. The study applies, among other methods, 3D topographical analyses on archival and digital materials. The theoretical framework is formed around Latourian Actor-Network Theory and Modes of Existence, and the anthropological economic theory of the late David Graeber. The purpose of the theoretical assemblage is to gain a perspective beyond the modern Western notions of society and economy. Accordingly, the geography of Central Fennoscandia can be populated with non-human agencies and entities, who would not have gone unnoticed from the local inhabitants. Most notably, post-glacial land uplift animates the geography, while also providing local archaeology with the tool of shoreline displacement chronology.

The narrative of the prehistory of Central Fennoscandia indicates that after agrarian forms of subsistence began to expand into the region during the 4th and 3rd millennia BCE, a constantly shifting frontier between agricultural and forager contexts persisted in the region for over four millennia. Regarding the politics of these communities, it is argued that the different modes of subsistence were ingrained in ideology. Subsistence ideology affected attitudes to labor and organization. This approach highlights the cultural division within the region. Analyzing mortuary practices as signs of respect and morality alleviates the division, while shifting the contrast to highlight the divergence between the multivalence of prehistoric mortuary practices and the singularism of Christian dogma.

This multivalence is further investigated by a comparative study of the material records of two regions within the Bothnian Arc. The results indicate that the local communal identities differed from each other practically throughout prehistory, even though they are frequently categorized under the same material cultures. It is expected that with more comparative studies, the pluralism of communities would only increase as more local traditions are uncovered. This conclusion makes the concept of society questionable in the context of pre-state collectives.

Keywords: agency, archaeology, burial traditions, digital humanities, identity, local communities, network theory, prehistory

Hakonen, Aki, Perämerenkaaren paikallisyhteisöt osana esihistoriallista maailmaa.
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Tiivistelmä

Tutkimuksen tavoitteena on laajentaa ymmärrystämme esihistoriallisesta maailmasta, tuoden sen lähemmäs yksilöä. Keskiössä ovat Perämerenkaaren kadonneet paikallisyhteisöt. Ymmärtääksemme niitä, meidän tulee ymmärtää laajempaa Keski-Fennoskandian esihistoriaa. Pääasiallinen aikarajaus on 5500 eaa. vuoteen 600 jaa. Tutkimuksessa sovelletaan, muiden menetelmien ohella, 3D-topografisia analyyseja arkisto- ja digitaalisiin aineistoihin. Teoreettinen viitekehys on koostettu latourilaisesta toimijaverkostoteoriasta ja olemassaolon ilmenemismuodosta, sekä edesmenneen David Graeberin antropologisesta talousteoriasta. Teoreettisen koosteen tarkoituksena on tarjota väylä modernien länsimaisten yhteiskunta- ja talouskäsitteiden läpäisemiseksi. Keski-Fennoskandian maasto voidaan näin kansoittaa ei-ihmistoimijoilla, jotka tuskin olisivat jääneet paikallisilta huomaamatta. Erityisesti jääkauden jälkeinen maankohoaminen elollistaa maaston, tarjoten samalla rannansiirtymiskronologian työkaluksi paikalliselle arkeologialle.

Keski-Fennoskandian esihistorian narratiivi osoittaa, että maanviljelyyn perustuvan elinkeinon laajennuttua seudulle 4. ja 3. vuosituhannen eaa. aikana, eri elinkeinokontekstien välinen jatkuvassa liikkeessä oleva rajaseutu oli olemassa yli neljän vuosituhannen ajan. Näiden yhteisöjen poliittiseen toimintaan liittyen, eri elinkeinomallit tulkitaan ideologiaan sidotuiksi. Elinkeinoideologia vaikutti suhtautumiseen niin työhön kuin järjestäytymiseen. Tämä lähestymistapa korostaa seudun kulttuurista kahtiajakoa. Hautaustapojen analysointi kunnioituksen ja moraalin ilmentymänä lieventää kahtiajakoa, siirtäen kontrastin korostamaan jakoa esihistoriallisten hautaustapojen monimuotoisuuden ja kristillisen dogmin yhdenmukaisuuden välille.

Tätä monimuotoisuutta tutkitaan edelleen vertaamalla kahden Perämerenkaarella sijaitsevan seudun materiaalista lähdeaineistoa. Tulokset osoittavat paikallisten yhteisöjen identiteettien eriävän käytännössä koko esihistorian ajan, vaikka ne usein luokitellaan osaksi samaa materiaalista kulttuuria. Verrannollisten jatkotutkimusten myötä yhteisöjen moninaisuuden odotetaan vain lisääntyvän, kun lisää paikallisia perinteitä tulee selville. Tämä johtopäätös asettaa yhteiskunnan käsitteen heijastamisen esivaltiollisiin ryhmiin kyseenalaiseksi.

Asiasanat: arkeologia, digitaaliset ihmistieteet, esihistoria, hautaustavat, identiteetti, paikallisyhteisöt, toimijuus, verkostoteoria

For Jenni and the kids

Preface

On an intercity bus ride a year ago, I had a few hours to think. It was a late autumn evening in Finland. Looking out the window I could only see my own reflection superimposed on ubiquitous darkness. I had just sat through two days of presentations on ongoing archaeological research in Finland. Everyone seemed to have the answers. My mind, on the other hand, was encumbered by one thought only: What do I actually know? With my doctoral dissertation inching toward its seventh year, was I any wiser for all the work and effort? What answers can I give? I took out a notepad and wrote the first draft for the title of this book: 'The Big Questions. Riddles of the Prehistoric Central Fennoscandia—Unresolved'.

A few months later, I received a book, 'Archaeological Theory in Practice' by senior archaeologists Patricia A. Urban and Edward Schortman (2019). Their advice: Do not be afraid to fail (p. 302). Too often people try to shield themselves from the appearance of failure. Yet, failure is the ultimate lesson, and at this preliminary stage in my career, my everyday work concerns learning new things in a constant barrage, mostly through failure. I expect to continue on this path for the rest of my life. I hope to relish every failure on the way. Not that I consider this work a complete failure, but rather as a composite of many exciting ones.

I have been aided by truly exceptional people. Obviously, my supervisors Vesa-Pekka Herva and Janne Ikäheimo deserve credit for their casual drive and extensive knowledge, and also patience. My closest colleagues Ville Hakamäki and Mirette Modarress supported me throughout much of the process, lending a pair of ears whenever I needed. Pre-reviewing this synthesis, Jari-Matti Kuusela provided scathing criticism, which helped me improve my arguments in ways I would not have otherwise even considered.

Eavesdropping on conversations between people far smarter than me, especially Jari Okkonen, Samuel Vaneeckhout, Matti Enbuske, Risto Nurmi, Tiina Äikäs, and Timo Ylimaunu, I became less ignorant on a range of subjects. By asking my help to navigate the nooks and crannies of ArcGIS, Karen Niskanen gave me the impetus to hone my spoken English, computer tech skills, and my teaching skills. In return, as if I had not gained enough, she taught me plenty about Fennoscandian rock art. My more infrequent contacts, including among others Vadim Adel, Peter Holmblad, Satu Koivisto, Lene Melheim, Teemu Mökkönen, Kerkko Nordqvist, Frida Palmbo, and Petro Pesonen, kindly provided answers to some of my most foolish questions.

As I was embarking on this path, my teacher-sages Kirsti Paavola and Eero Jarva offered much practical advice which resonates with me to this day. In the early stage, my brief encounters with Ingela Bergman and Lars Liedgren were inspiring, as well as with Per Ramqvist, who graciously reviewed my grant applications and wrote letters of recommendation. On the same note I also wish to thank Jari Okkonen, Matti Enbuske, Janne Ikäheimo, and Vesa-Pekka Herva for providing me with recommendations accompanying the 48 grant applications submitted to fund this project.

On the funding front, I am deeply grateful for the six grants that I received during the seven years. Early in 2014, a two-month grant by the Swedish-Finnish Cultural Foundation and a six-month grant from the University of Oulu Faculty of Humanities gave me a head start. Grants by the University of Oulu Scholarship Foundation (for three months in 2015), Otto A. Malm Foundation (for six months in 2016–2017), and Scholarship Fund of the University of Oulu (for three months in 2017–2018) carried me through some frugal years. The final nails were handed by the North Ostrobothnia Regional Fund of the Finnish Cultural Foundation, whose generous grant (ten months in 2019–2020) allowed me to focus on finalizing this dissertation.

During the process I experienced many periods of intense activity and lagging hiatus. Partly to supplement my limited funding, but mostly to satisfy my curiosity, I also worked on other projects and undertakings, all in all for 19 months. I wish to thank my part-time employers at the university, Titta Kallio-Seppä, Tiina Väre, Jari-Matti Kuusela, Ville Hakamäki, and Vesa-Pekka Herva. Also I would like to thank my architect friend Harri Ryynänen, with whom many days and some cold late evenings were spent tracking the green stroke of the terrestrial laser scanner. Also, as of 2018, I have worked part-time for the underwater archaeology company Subreering Ammattisukellustyöt Oy, alongside the specialists Simo Nyrönen, CEO, and Pekka Paanasalo, who provided me the experience of glancing the world below the surface.

Before relocating with my family to Tampere in 2018, the GIS-laboratory of the archaeology department was a safe haven with its own distractions. Among the most fruitful distractions were theoretical discussions with Jari Okkonen, who influenced my thinking and outlook in more ways than I can consciously conceive.

It was Jari who suggested in early 2016 that I look up anthropologist David Graeber. Graeber, who mixed activism, scholarship, and nuanced theoretical thinking, shaming immovable paradigms with his analyses, had a huge influence on me. The books I initially got my hands on, ‘Fragments of an Anarchist

Anthropology' and 'Debt—the First 5,000 years', became major building blocks in my theoretical framework. Graeber's treatise on the bullshitization of jobs, with the expansion of pointless corporate bureaucracy stifling creativity and making people feel useless overall, proved a great comfort to me when the automated student credit system of my university labeled me "passive." With a theoretical concept as elegant as bullshit at hand, I understood the gravity of the trend, while appreciating the absurd humor of it. Graeber's writings, recorded talks, and example also encouraged me to participate in activism. For all this, I am forever indebted. On September 2nd, 2020, David Graeber died at the age of 59.

Also on a personal note, I am truly blessed by the support of my family, on my side and my wife's. Sadly, the first feline associates I learned to comprehend, Vinski, Väiski, and Peppi, passed prior to the completion of this work, but they never cared much for books anyway. A welcome addition to our family is Tilda, the baby-daughter of my sister-in-law, who may just someday come across her name here.

I am grateful for carrying within me the kindness of my grandmother, Laina Hakonen (1924–2016) and my grandfather Aulis "Ali" Salmi (1929–2018). Just talking with my grandma was simply a pleasure, and sometimes, immersed in thought, I realize that I have drifted into one of our conversations. And I wonder whether my choice of profession was influenced by grandpa pointing at a small circle of loose stone blocks on solid bedrock and telling the six-or-so-year-old me, that it was in fact a Stone Age court site. It must have been a joke, I now realize, but at the time I believed that actual cave people had sat on those hallowed rocks.

I also wish to thank the Chief Museum Preparator, Reijo "Reippa" Pasanen, of the Finnish National Museum, who during my internship long ago, advised me not to pursue a career in archaeology. Additionally, my sincerest thanks to Jesper the dog for putting things into perspective.

Most of all, I would not have wandered down this path without my family, my wife Jenni and our cat-children Aatu and Vilppu. The two rascals have not been completely supportive and have done their best at every turn to sabotage my work. But they also kept me clear-headed. Jenni too. She is my inspiration, my purpose. For her I do whatever I do, just so that she might be a little prouder of me.

Aki Hakonen
Tampere, Finland
October 11th, 2020

Abbreviations

BCE	before common era
BP	before present (1950)
ca.	circa, ‘approximately’
calBC/AD	calibrated radiocarbon date, corresponding with BCE/CE
CE	common era
cf.	confer, ‘compare’
CWC	Corded Ware Culture
DEM	Digital elevation model
e.g.	exempli gratia, ‘for example’
etc.	et cetera, ‘and so forth’
FHA	Finnish Heritage Agency
i.e.	id est, ‘that is’
m a.s.l.	meters above sea-level
NLSF	National Land Survey of Finland

Original publications

This thesis is based on the following publications, which are referred throughout the text by their Roman numerals:

I Hakonen, A., Hakamäki, V. & Kuusela, J.-M. (2017). Observing social change on the Bothnian Bay coast in the 1st millennium BC: The burials of Tahkokangas and the community of the Oulujoki river estuary. *Assemblage*, 15*,* 15–27. https://assemblagejournal.files.wordpress.com/2017/05/hakonen-et-al-to-submit.pdf

II Hakonen, A. (2017). Shoreline displacement of the Finnish Bothnian Bay coast and the spatial patterns of the coastal archaeological record of 4000 BCE – 500 CE. *Fennoscandia archaeologica*, 34*,* 5–31. http://www.sarks.fi/fa/PDF/FA34_5.pdf

III Hakonen, A. (in press). Interpreting prehistoric labor north and south of the forager-agricultural frontier in Central Fennoscandia, Northern Europe. *Arctic Anthropology*.

IV Hakonen, A. & Hakamäki, V. (2019). Living with death: what moral consideration of mortuary practices reveals about the plurality of worldviews in the multi-millennial past of Central Fennoscandia. *Time & Mind*, 12(4), 287–304. https://doi.org/10.1080/1751696X.2019.1681744

V Hakonen, A. (2021). Communities Beyond Society: Divergence of Local Prehistories on the Bothnian Arc, Northern Europe. *Open Archaeology*, 7(1), 211–230. https://doi.org/10.1515/opar-2020-0132

Papers II, III, and V were sole-authored. Paper I was conceptualized in concert with my co-authors with the writing process being 90% my own work. Paper IV was conceptualized and written jointly with 80% of the work performed by myself. Both papers received significant input from the co-authors prior to submittal.

Ville Hakamäki, PhD, (co-author on Papers I and IV) is a Finnish archaeologist who received his doctorate from the University of Oulu in 2018 and is currently the lead archaeologist of the Museum of Northern Karelia.

Jari-Matti Kuusela, PhD, (co-author on Paper I) is a Finnish archaeologist who received his doctorate from the University of Oulu in 2013 and is currently the lead archaeologist of the Provincial Museum of Lapland.

Contents

1 Introduction

Archaeology is inherently inconsistent. Material records are filled with gaps, some caused by lack of research and some by naturally occurring destructive processes. Sometimes the record in the ground disappears completely and re-emerges on a fluke. Sometimes its abundance overwhelms the hapless researcher. Archaeologists also see the same evidence with different eyes and thoughts. The theoretical frameworks, existing in every one of us, academic or not, guide our thinking along unique tracks, or sometimes into unpaved thickets. Archaeology meets at the crossing of these different elements, the material and the mental. It is successful when the two are intertwined into a convincing argument where assumptions that lie at its roots are brought to light. Absolute truth eludes, while interpretations proliferate.

In this dissertation I present not only the end result of my doctoral project, but—in accordance with a Latourian framework—also the process, with its ups and downs, dead-ends, and open freeways. The operative term here is *local community*, which is pursued mostly with 3D-topographical and landscape perspectives. In making sense of the data, a customized theoretical toolkit greatly influenced by the Actor-Network Theory is applied. Used hand-in-hand, 3D-topography and Actor-Network allow us to transcend traditional boundaries that often separate themes and modes of existence. Instead, we can study collectives earth-bound.

During the research process that culminated in this book, along with the five related articles, my intent has been to try out different scalar perspectives to see whether they open up new vistas into such communities in prehistory. By initially understanding these communities as building blocks of prehistoric society—a concept whose implausibility we will discuss later on, while continuing to struggle with it throughout the book—I wished to gain more insight into the fabric of reality within these worlds. In the process, I learned how my Western worldview with its Enlightenment heritage, while offering the background from which to conduct research, also acts to impede my understanding. Throughout this process, I have tried to force my way through some of these mental obstacles, and I hope you the reader will have the patience to follow me. Or at the very least I hope you will enjoy watching me squirm.

While the study relates overall to the prehistory of Central Fennoscandia (see geographical definitions in the introduction to Chapter 4), the focus is on the Bothnian Arc. This coastal strip stretches northeast from Kokkola in Finland, arching back southwest from Tornio, reaching Lövånger in Sweden on the opposite

side of the Kvarken strait. The Arc surrounds the Bothnian Bay, where issues of glacial history, the abundance of Neolithic materiality, and the puzzles of later prehistoric developments have formed a stimulating arrangement of regional heritage, which continues to generate more questions than answers.

A vast array of archaeological research has been conducted in the region. Also, the vast majority of it has been geographically and temporally delimited based on artificial borders, whether national or municipal, or strictly narrowed archaeological eras. There have previously been no up-to-date overviews of the region's long-term borderless prehistory. This dissertation provides the first, but its actual purpose lies deeper than that. The overall aim is to argue for a reorientation of the perspective of archaeological research, from abstract societies to communities on the ground. This way archaeology could account for a pluralism of local identities, instead of perpetuating the projection of modern nation states and their definite borderlines onto prehistory.

In addition to this synthesis, the study consists of five research articles: first, an introductory paper pertaining to a narrow case study of a Late Bronze Age local community on the Oulujoki River estuary which acts as the launch pad for this dissertation (Chapter 7.2; Paper I); second, a method-oriented paper verifying the local shoreline displacement chronology (Chapter 4.3; Paper II), and three thematic papers. The themes that are explored are labor activity (Chapter 6.1; Paper III), mortuary practices (Chapter 6.2; Paper IV), and material identity (Chapter 7.1; Paper V). Each theme is approached with its own geographical focus, zooming in from the widest perspective of Central Fennoscandia to the narrowest of a region surrounding a river estuary. A diligent reader may find it helpful to scan through the research articles first in the numbered order and proceed with the synthesis after that, although I hope that the synthesis will stand on its own. Alternatively, the conclusions of the papers can be read in their summarized form in Chapter 8.

The themes were analyzed with an emphasis on controversies, meaning situations where the underlying logic is not shared throughout time and space. In Paper III, focusing on labor activities in prehistoric Central Fennoscandia, I explore the expended effort in producing extra-subsistence-related material culture from two cultural perspectives to see how subsistence contexts and labor roles interact. I posit that conceptualizing subsistence as an ideological movement instead of a mere practice offers more insight into why subsistence is reflected in forms of material culture that are not directly related to subsistence practices.

The two binary ideological movements are conceptualized here as production and procurement. The ideology of procurement relates to the moral imperative of

acquiring and receiving what is given by the multitude of agencies within the environment and landscape. A distinct ideological movement is that of production, which was accentuated in the region by apparent prehistoric upheaval of the Neolithic period. In production, subsistence and surplus were sought by owning and modifying the environment and landscape so that livelihood was not merely received but made. In the production context, leadership roles and hierarchical relations may have been organized to support increased production, but as we will see, the same seems to have happened in the other context as well, although periodically rather than consistently. The two ideological movements can be interpreted as diverged cultures, which coexisted in the region until fairly recently. The ideological heritage of procurement lives on with the northern culture of the indigenous Sámi (see e.g. Helander, 2000), while the agricultural-industrial state societies of the south amplify the legacy of production.

In Paper IV, we, myself and Ville Hakamäki, focus in on the northern half of Central Fennoscandia and turn the analysis to mortuary practices. Here, we followed the data all the way to a controversy extending to the present day, with modern mortuary traditions differing from prehistoric ones in terms of diversity and logic. While there seems to have been no overruling ways to conduct morally appropriate burials during prehistory, with Christianity uniformity eventually became a morality in itself. We argue that prehistoric communities were, in effect due to their proximity with death, more relaxed in their attitude to it than Christian society and subsequently we in the modern society tend to be. I hope that the archaeological perspective lays bare the clear flaw in our own Western culture in how we relate to death in large part through fear and denial, a sentiment also expressed by archaeologist Marja Ahola in her recent doctoral dissertation (Ahola, 2019: 5–6).

Paper V narrows further in on the northeastern Bothnian Arc, where the archaeological assemblages of two separate coastal regions are compared. The objective here is to find different material expressions which communicate local communal identities. The timeframe of the paper extends from the Late Mesolithic to the Late Neolithic, but in this synthesis I present an extended analysis, which is based on my original collation of research material, from the Late Mesolithic to Mid-Iron Age. Paper V along with Chapter 7.1 shows how the two regions maintained their own material identities, and I suspect that should the same analysis be repeated on other neighboring regions, the results would reveal more and more regional nuances, which represent strictly local traditions.

The five papers are compiled together in this synthesis. The synthesis consists of ten chapters including the introduction and conclusions. In Chapter 2, I will clarify the objectives of the study and present the main materials, their sources, and the digital applications which composed the frames in which the research was conducted. Chapter 3 explores the theoretical framework, within the confines of which the analyses—ideally—pass through to become interpretations. The theoretical discussion is expansive and pertains to many forms of past and present activities.

In formulating the theoretical toolkit, I have chosen to minimize the role, as well as I can, of some of the most anachronistic models that I have found to be regularly employed in archaeological research. Mainly, these are the concepts of society and economy. This was not my objective from the onset, as I was intent early on to apply the two structures to this specific prehistoric context. Eventually I was introduced to the works of two theoreticians, which led me to redirect my orientation. The first is the sociologist Bruno Latour, known mostly for Actor-Network Theory and less for the subsequent Modes of Existence. The second is anthropologist David Graeber, who continued Marshall Sahlins's endeavor of making economics relevant in pre-monetary contexts. Equipped with fragments of the knowledge of others, my aim is to approach as close to the individual as is theoretically feasible in such a large-scale study. Ultimately the scope is fixed on the level of local communities, which I apply as the preferred focus of study.

In Chapter 4, I briefly summarize the geography, along with its definitions, and environmental factors that have affected humans. Furthermore, I discuss how both geography and environment can be understood as living entities. Finally, I present and slightly expand on Paper II, in which I have verified and refined the shoreline displacement chronology of the region. In Chapter 5, before focusing on the main issues of the other papers, I summarize the region's prehistoric narrative, which I have mostly collated from prior research. In some cases I propose small amendments based on my observations.

Chapter 6 presents the overviews of Papers III and IV, which pertain to issues of cosmology ingrained within the everyday lives of communities. These issues relate mainly to work and death, and the way people shifted between hierarchical and anarchistic organization. The underlying response to death and the deceased is argued as respect, which is surprisingly controversial in the context of archaeological research. Chapter 7 continues with a refocusing of the perspective on local communities themselves, first with the analysis presented in Paper V extended to a longer time span, and by revisiting the subject of Paper I, which is

where the process began. Again, the results of the papers are summarized for the reader's convenience in Chapter 8.

Finally in Chapter 9, I discuss the implications of the study not only on how we should deal with the concept of prehistoric society, but also on how the study reflects our current times, and how learning from the past may offer insight for mending some of the defects experienced in current society. In addition several significant and vulnerable scientific nodes in the archaeology of the wider Fennoscandia are discussed and criticized. I also try to illuminate the most apparent defects in my own methodology, while acknowledging that in many ways my objectives, the local communities themselves, still remain out of reach.

All in all, I hope this book manages to communicate the process of this particular archaeological research project. Instead of a strict academic writing style, I am applying a style more suited for myself, which some have characterized as journalistic or essayist. The research papers are nonetheless composed in academic style. Perhaps this work will inspire someone and even motivate some to pursue a similar path, wherever it leads. All errors and inconsistencies in this synthesis are mine alone. I maintain no pretense of absolute truths in this study, and where my arguments appear as such, they should be taken with a grain of salt. There are only so many ways in which to say "possibly", so I may have taken a few shortcuts to achieve a semblance of brevity.

2 Objectives, materials, and the process

This chapter describes the background and aims of the dissertation. Hopefully, the reader will gain an understanding of the reasons why I chose this particular subject and of the practical labor that I occupied myself with for the duration of the study.

For each site mentioned in the text, I will either demark its registry code in squared brackets (for sites in Finland) or its municipality and registry number (for sites in Sweden). These codes can be used to find the site in the corresponding national archaeological site registry.

I found the term *muinaisjäännös* (FI) and *fornlämning* (SWE) surprisingly difficult to translate, as they refer to both sites and structures. Archaeological site is applicable when referring to individual places, yet when discussing different categories *site* becomes too abstract. The various terms I considered were *ancient remain*, *monument*, *relic*, and *heritage site*. Yet, all terms proved problematic, with remain being easily confused with the verb indicating the act of remaining, monument associated with monumentalism, relic evoking a loose object, and heritage site being a managerial term. Throughout this dissertation I reserve the term *remnant* as the root when referring to structural material categories. By *remnant site* I refer to similar categories of archaeological sites.

2.1 Research objectives and their formulation

1. Gain an understanding of long-term changes that took place in Central Fennoscandian prehistory from 5500 BCE to 600 CE.
2. Apply multi-scalar case studies with different perspectives to assess pluralism within the prehistoric world.
3. Evaluate the usefulness of the concept of prehistoric society as a unit of analysis and propose an alternative.

This dissertation employs different scalar perspectives in studying the prehistory of the Bothnian Arc and its place in the wider Central Fennoscandian context. The aim is to find new access points into prehistory. These access points manifest in material culture, which, when applied with a specific theoretical perspective, can yield clarifying interpretations of lived lives. As I said in the introduction, the overarching theme is an attempt to weaken the disruptive effect of national borders on local prehistory. These borders have in many studies acted as geographical limits,

which has caused the prehistories of Finland and Sweden to diverge unnecessarily. In the following section (Chapter 2.2), some practical divergencies are discussed.

Applying multiple scalar and methodological perspectives became an increasingly fulfilling learning process. While strict scientific specialization offers a clear pathway to a career, it also imposes severe restrictions on the individual and what they can do. Collating a wider ranging yet arguably more superficial toolset at the beginning of one's research career should offer, figuratively speaking, resistance to myopia.

The original focus of the study was strictly limited to analyzing local differences between river estuaries, with the timeframe set between 1500 BCE and 600 CE. But early on it became clear that in order to truly understand events that are indicated by the archaeological record, the wider context had to be assembled first. Studying a chosen timeframe by disregarding its contemporary past means disregarding the origins of the subject. And the more I learned about Fennoscandian prehistory, the more evident it became that this study had to be extended much further back in time. As will become apparent in Chapters 5 and 7, the majority of evidence for local differences between neighboring groups are to be found in the Neolithic record, prior to my original timeframe. In the end, the timeframe of the study extended to 5500 BCE with the end date of the case studies varying from 600 CE to the present day.

The expansion of the studied timeframe meant that the original plan for the dissertation and its methodology had to be reformulated. What was once conceived as a monograph, with a strictly local single-scalar focus, became a collection of articles, all with different scalar perspectives, and accordingly with different methodologies and geographical foci. The resulting five-pronged approach aims to achieve the objectives presented at the beginning of this section.

2.2 Digital research components

Due to the vast timeframe of the study, efficiency became a key issue. This issue was resolved by sheer luck and the hard work of others. During the 2010s Finland's archaeological archives were rapidly digitized. A national archaeological site registry had been compiled in previous decades and it is continuously maintained and updated by the Finnish Heritage Agency (up to 2018 known as the National Board of Antiquities). The registry—a public-access internet domain since 2009 (Haimila, n.d.) —contains site-by-site information, including classifications of remnant-types and coordinate data, and it acted as my main source of spatial

information. The registry is available as a downloadable shapefile, which allows it to be overlaid with other mapping data in GIS-applications. Short descriptions of each site can be found in the online version of the registry (see www.kyppi.fi).

Until recently, the registry was riddled with errors and inconsistencies. One of its more debilitating quirks was the classification *remnant group*, an umbrella term for sites where several categories of remnants exist. For example, as I frequently noted, if three cooking pits lay in the vicinity of a trapping pit, the site could be categorized as a remnant group. This would mean that Boolean searches and classifications for both cooking pits and trapping pits would omit the site. In 2015, over 1,250 remnant group sites were registered out of the total of 45,000 sites. The relatively small number represented some of the most significant archaeological sites in Finland, since elaborate and widely excavated sites usually include more than a single remnant-type.

Because of such intricacies, the registry was only useful when relevant site data was collated one by one into a customized database. Most of the excavation reports were initially accessible only at the National Board of Antiquities archives in Helsinki. With no clear idea of which archaeological sites were more important than others, the site descriptions in the online registry and data published in research articles became my starting point. This was in 2014, a year during which I spent most of my time collating my own database from the scraps of information.

Now, during the writing of this synthesis, not only has the official registry become partly restructured, with most of the remnant groups divided into classifications of all the individual remnant-types contained within, but around 90% of all excavation, survey, and inspection reports have become available online. Also, the majority of artefact lists from excavations and surveys are downloadable from the same source. The rapid development of the *digital cultural environment portal* maintained by the FHA (www.kyppi.fi) has greatly expanded the potential of Finnish archaeology.

Its inherent problem is that most of the data is in Finnish, with a minority in Swedish. Thus, prior to digital translation tools reaching sufficient accuracy, the online database is truly accessible to the less than six million people worldwide who are proficient in Finnish. This places the obligation to publish prior research firmly in the hands of Finnish archaeologists, a pool, which—although actively publishing in international journals—is not known for its numbers.

As my intent was to break down the barriers caused by anachronistic national borders, in my study I implemented both the Swedish *Fornminnesök*-registry and the Norwegian *Askeladden*. Both are national archaeological site registries of

varying quality. Attending numerous Scandinavian conferences and meetings during the last seven years, I got the chance to hear from many archaeologists in Sweden and Norway about their experiences with the respective national registries. Swedish archaeologists commonly stated that *Fornminnesök* should be applied in research only with diligence and extreme caution, due to the vast number of erroneous sites. Yet they acknowledged the registry's merits in allowing efficient big-picture overviews of remnant-type dispersals. The latest *Fornminnesök*-registry that I downloaded and customized is dated March 2017.

Swedish excavation reports are more difficult to come by than Finnish ones, since in Sweden the archives are managed by regional museums, which all have their own procedures. In Finland, the FHA manages all the archaeological records and finds, which has allowed the formation of a centralized public access archive.

The Norwegian archaeologists that I met with expressed mostly frustration and disapproval regarding their national registry. Problems with disjointed classifications, sometimes jumbling together vastly different remnant-types, for example Neolithic dwelling depressions and historical houses, proved especially confounding to me. The *Askeladden*-registry that I downloaded dates back to January 2016, and whether its classification structure has improved since is unclear, although the latest negative reviews, describing the registry as a complete mess, I heard in early 2019. Nevertheless, the registry offers a glimpse into the spatiality of some material cultural traditions, even though getting acquainted with the logic of the classifications involved considerable effort.

The most important thing in using these three national registries conjointly is the understanding of the different remnant-type classifications and the differences in the representation of sites. Most of the remnant categories of the registries were formulated independently from other registries, causing incompatibilities between them. Other incompatibilities include even the data structure. For example, *Muinaisjäännösrekisteri* contains point data for each site, an accompanying polygon-based area data for most sites, and sometimes sub-point data within sites for each documented remnant. In *Fornminnesök*, on the other hand, sites are sometimes represented either by a point, a polygon, or a line, each with their own appearance in GIS. Sometimes point data demarks single remnants within sites, with no spatial indicator for the site as a whole. I tried studying *Askeladden* to see how its logic differed from the two other registries, but I ended up only confusing myself, and I relegated my use of it only for general overviews. Learning to use these registries by combining the GIS-databases with the online versions and using

them simultaneously to supplement the weaknesses of the other format, constituted one of the biggest technical challenges of the study.

The mapping data that I used during the process included terrain models of Finland and Sweden, produced by the National Land Survey of Finland (NLSF) and the Swedish *Lantmäteriet*. Beginning with 2008, the National Land Survey of Finland conducted aerial lidar mappings. From this data, 2-meter resolution elevation models have been produced, which covered most of Finland by 2014. These proved extremely useful in conducting digital GIS-surveys of the landscape and environs of individual sites. Such surveys acted as the background for the planning of publications and informed my decision-making when choosing the most relevant sites. Also both the raw lidar data and the processed hillshade-maps were used in site-specific analyses. Operating these adaptable datasets can best be characterized as 3D-topographical research, in contrast with simple topography, where elevation data is present in a more rigid abstract form.

In wider overviews, where the perspective encompassed the whole of Fennoscandia, the 250-meter resolution regional elevation models distributed by the open-source DIVA-GIS-resource were used. Since the Swedish lidar data was difficult to access, the most accurate elevation model that I used for the Swedish Bothnian Bay region was 50 m in resolution, produced and openly distributed by Landmäteriet. It should be noted that national borders do not only desynchronize the archaeological record, but also act to restrict the availability of geographical data.

The GIS-applications used here were ArcGIS Desktop (v. 10.2.–10.6., 2013–2019) and QGIS (v. 3.6.0-Noosa – 3.12.0-București, from 2019 on).

2.3 The 7-year process

I began planning this dissertation just prior to my graduation as a Master of Arts in November 2013. My first research plan originates from the previous month, and I gave my first presentation of the plan in December. The official starting date, however, was in March 2014, when I was accepted to the newly formed University of Oulu Graduate School (UniOGS). I got my first two funding grants immediately after, one from the Faculty of Humanities for six months and the other for two months from the Finnish-Swedish Culture Fund. I received four more grants between 2015–2019, ensuring funding for 20 months. In total, the funding for this dissertation supported research for 2 years and 4 months. Subsequently, a four-year plan turned into seven.

The dissertation took off from where my master's thesis ended. The focus of the previous thesis was originally on the 3D-laserscanning documentation of the stone settings at Tahkokangas [564010023] in Oulu. Eventually I became more interested in the surrounding archaeological context. The thesis offered me a glimpse into a prehistoric local community, the perspective I aimed to maintain during this doctoral dissertation. Paper I—which was originally composed mostly as a marketing tool in the pursuit of funding—is based on my master's thesis, with slightly expanded interpretations. Paper I was finalized in June 2014 and published in April 2017.

As previously mentioned, the current dissertation was originally planned as a monograph, which would have extended the analysis in Paper I, performed on the Late Bronze Age (see chronology in Table 3) Oulujoki River estuary, to river estuaries around the Bothnian Arc. This would have been essentially the analysis that archaeologist Lars Forsberg had argued for 15 years earlier, with the aim of constructing local societies from the ground up (see Forsberg, 1999). With a focus on *assemblages of sites*, and the keyphrase network theory in my head, I came across, in the sociology section of a Helsinki bookstore, Bruno Latour's 'Reassembling the Social. An Introduction to Actor-Network-Theory' (2005). Latour's definition of reassembling and network had nothing to do with what I had in mind. However having previously introduced myself to Social Network Analysis (see e.g. White, 2008), and finding little adaptability in it for my context (see Chapter 3.1), probably owing to my theoretical inexperience, I decided to follow up on this new lead.

For a month, during the July-August heatwave of 2014, I immersed myself in Latour's book. It took me four weeks, eight hours a day, five days a week to finish the book, which I struggled to comprehend. Especially the use of familiar words in completely alien use was daunting. Looking back on it, this is not surprising, since French sociology, especially when translated to English, is not known for its clarity. I took comfort in the words of anthropologist Tim Ingold, whose commentary on Actor-Network Theory resonated with me deeply.

> 'You are indeed a master of lofty thoughts, But I cannot, for the most part, understand a word of what you say.'
>
> – Tim Ingold (2008)

Nevertheless, despite the initial difficulties, Latourian Actor-Network Theory became the basis for my theoretical perspective, as I detail in the next chapter.

Instead of focusing on simplistic cultural connections, or tracing a network based on morphological material connections, I now began understanding deeper connections between actions and actors. Now, not only was prehistory composed of passive artefacts and the active humans who deposited them, but also of active objects, ideas, and living beings vastly outnumbering humans. All these organic and inorganic actors affected individual human actions in ways which were discernable in the archaeological record. With the integration of the Entanglement perspective (see Chapter 3.7), as formulated by archaeologist Ian Hodder (2012), I began to understand how these networks of actions functioned in the archaeological scale of time.

Due to these and subsequent theoretical readings, and on the advice of colleagues and supervisors, I eventually transformed the dissertation from a systematic monograph to thematic article-based format. The expansion of my theoretical thinking began highlighting certain themes, which became more relevant than the systematic approach that I followed until late 2015. Nevertheless, a systematic assembling of site information into a personal database, that I conducted mostly during the first year, gave me the material that allowed the thematic pivot. The theoretical toolbox that had taken shape made the pivot a necessity.

Between November 2015 and February 2016 I composed Paper II—published in Autumn 2018—in which I subjected the local shoreline displacement chronology to rigorous testing. I had used the chronology which derived from my MA supervisor's, Jari Okkonen's, licentiate thesis (1998a) and doctoral dissertation (2003), in my master's thesis and in the subsequent Paper I. While the displacement chronology itself turned out to be surprisingly current, it exposed a major flaw in the way I intended to use it. Cooking pits, which comprise much of the archaeological record of the Bronze Age and Early Iron Age and are a major resource for the study of contemporary events, turned out to be overwhelmingly located at non-systematic distances from the shoreline. Thus, as only a handful of cooking pit sites have been dated by other means, their chronology is exceedingly crude, and only partly refinable in regions of relatively flat terrain, such as the Ostrobothnian coast of Finland. Thus, in a systematic comparative study between regional site assemblages, the Bronze Age record would be excessively flawed (see Chapter 7.1).

This finding, also noted by archaeologist Petro Pesonen (2016), was the final nail in the coffin of my initial systematic approach focusing strictly on the Bronze Age. This freed me from pursuing a dead-end and encouraged me to eventually

recompose the dissertation into the three themes—labor activities, mortuary practices, and material identity—with different geographical and theoretical perspectives briefly summarized in the introduction.

Composing Paper III was the most difficult prospect of all, since it involves the largest geographical scale along with a long-term narrative. The paper was originally inspired by a reviewer's criticism of Paper II, in which the paper's original overview of the region's prehistory was deemed insufficient. The analysis of Paper III was conducted mainly between early and late 2016, during which I formulated the logic driving the interpretations. However, the paper was rewritten multiple times until early 2021. The paper achieved coherency only after omitting the overview of the region's prehistory, which is what initiated the paper. Instead, the overview is presented in more detail in Chapter 5 of this synthesis.

The data gathering and analysis of Paper IV were conducted between February and June 2017, and the first draft of the paper was completed by December. The article was submitted in April 2018 and it was published in October 2019. While composing Paper IV around September 2017, I began the gathering of data for Paper V. This involved composing summaries of 1,245 archaeological sites and their changing landscape phases. The dissertation was left on hiatus between February 2018 and March 2019, due to my teaching obligations and the conduction of a field research project at the site of Tainiaro in Simo, an apparent Late Mesolithic cemetery, which will be the focus of my post-doctoral research. The data gathering of Paper V was continued in August 2019, lasting until the end of the year. Both Papers III and V, in their latest form, were submitted in May 2020, and last revised in March 2021. This synthesis was written between February and October 2020, and slightly revised based on peer reviews in March 2021.

3 Forming the theoretical toolkit

Now that we have an understanding of the material at hand and we have stated some of the questions we wish to ask of it, we will turn to the framework which acts as the basis and main conduit for our interpretations. In Paper IV I have used the metaphor of interpretational key (borrowed from Latour, 2013: 48–53, 57–58) for highlighting how theory affects interpretation. The key can be understood in many ways, although I prefer either musical or cryptographic key. A physical key fits only a corresponding lock, but musical and cryptographic keys, when applied to the wrong set, always produce an interpretation, whose coherence reveals whether it is pertinent or not.

Both the problem and the intrigue with archaeology is that a plethora of scenarios can be interpreted from the same source by changing the key. As truth in archaeology is practically unattainable, the durability of a given interpretation is based on its felicity conditions, meaning, what needs to be stated in order for an interpretation to be valid and appropriate in the context (see Latour, 2013: 53–56). After this chapter I hope the reader will be afforded with a set of interpretational keys useful in the following chapters, with an understanding of their felicity conditions. The main narrative involves finding alternative approaches to society as a unit of analysis, and economy as an explanation for various actions.

Theoretical perspectives are always enrooted in their own time. During the late 19th century, when nationalist and colonialist contexts were predominant in archaeology, cultural evolution formed the basis for theoretical thinking. Today with globalization, the Internet, electric grids, freight and airline traffic, and ubiquitous webs of asphalt, network theory makes sense since it is rendered practically visible. The coronavirus pandemic that began in late 2019 only highlights the relevance of networks. At the initial writing of this paragraph, on March 19th, the number of confirmed COVID-19 cases had reached 220,000, with nearly 9,000 deaths globally, a tragedy unfolding. When revising the paragraph a year later, the confirmed cases had surpassed 117 million, with the official death toll a mind-numbing 2,600,000, despite all the countermeasures around the globe. The total extent of the virulent network remains to be seen, as well as its ultimate effects, whether short-term or for years to come. Yet, let us go back to 2014, when the world was a different place, at least with 2020 hindsight.

3.1 Failing to apply Social Network Analysis and modern economic theory

Because the study from the onset involved relations and connections within a hive of archaeological sites, network theory became an early necessity. Studying local communities involves significant difficulties. In this study theory and method are intertwined, one useless without the other. This derives from a seemingly simple—yet astoundingly complex—question. How should spatial boundaries of analysis be defined? It is easy to claim that sites A and B are spatially and temporally connected and thus represent the same local community, but to prove it is more difficult.

This is a common problem in archaeological network analyses. Similar questions arise. For instance, where do site assemblages end? Which is the strongest connector: time, location, remnant morphology or artefacts? These questions forced me to step outside my previous comfort zone and see how issues of relationship have been handled in other disciplines. The following is not meant as a general overview of network theories, but merely the path I followed, along with dead-ends and detours, which others might find more navigable than I did during the process.

The most obvious place to look was sociology. Previously, in my master's thesis I had used sociology merely to enrich archaeological interpretation, but not as a catalyst for analysis (see Hakonen, 2013). The material evidence provided frames and outlines, which stood as the limits for social interpretation. Thus, the social interpretation was not specifically based on the archaeology, but archaeology dictated the direction for interpretation, which derived from general sociological and cultural anthropological theories. The validity of this approach is arguable (the flaws become apparent once we turn to Actor-Network Theory), yet one may argue that it is a common approach to theory especially for pre-graduate students of archaeology.

With the previous experience, it was natural to proceed on to theories about connectedness and networks, which are abundant in sociology. So abundant that an all-encompassing review requires a book or two of its own. For a short introduction the reader is invited to refer to archaeologist Carl Knappett's book, 'An Archaeology of Interaction' (2011: 15–36).

The first theoretical models to catch my eye were from several articles related to the Scandinavian Viking Age by Søren Sindbæk. In these, Sindbæk produced network maps of sites connected through materiality. The tracings identified several regional nodal points central in the funneling of maritime trade (see Sindbæk 2007a;

2007b; 2012). These papers and the idea of systematically charting material networks led me to my first impasse.

Sindbæk's approach was mostly inspired by Social Network Analysis (SNA). Its theoretical endgame is to track threads that weave networks and ultimately form webs of personal associations (see White, 2008). The attributes of different subjects are identified and traced to other subjects who share similar attributes. The attributes provide the adhesive that links subjects into relations. The relations, when sufficiently traced, form networks of connections. This approach has been particularly useful in studies of human contact tracing (e.g. COVID-19) and in studying material linkages. Archaeologists have made significant contributions in applying the method (see e.g. Knappett, 2013).

Nevertheless, two issues caused me to abandon this approach early on. First, the emphasis on social relations and second, what I saw as the inapplicability of economic theory to prehistory. SNA is based on the charting of social relations (see e.g. White, 2008). Thus, I became disillusioned with its applicability on materiality. As my initial focus was on sites and the remnants within them, my concern was that what applying SNA would actually result in would merely be a network visualizing the differentiation of dissimilar artefact and remnant categories. This way, closely situated and contemporaneous mortuary, dwelling, and production sites would be placed in separate networks.

Also, there was the issue of economics. The Viking Age differed considerably from the previous periods in terms of how trade was manifested within the northern maritime setting. This was a time of state formation and early urbanization (see e.g. Hodges, 1982; Thurston, 2002; Haywood, 2015), whereas the preceding periods in the north were, by all evidence, not (see Chapter 5.2). In my view at the time, networks in the pre-Viking Age northern context could not be assumed to have passed categorically through centralized nodes. This turned the yet-to-be-drawn coherent nodal network of the Bothnian Bay, which existed only in my mind, into a cluttered mess of linear connections splattered all over the map.

It did not help that early on in the process I began ploughing through economic textbooks at the university library in search of applicable theories for understanding pre-capitalist economics. I had already acquainted myself with Marcel Mauss's seminal work 'The Gift' (1954) and Marshall Sahlins's 'Stone Age Economics' (1972), which together gave me my initial frame of reference for prehistoric economies. In April 2014, I and Ville Hakamäki, PhD since 2018, hosted a session titled *Exploring socio-economic mechanisms in the past and present* at the 14th conference of the Nordic Theoretical Archaeology Group (NTAG) in Stockholm.

In preparation for the session, and our joint presentation *Prehistoric coastal communities: the economic network and its social implications*, my considerable but undocumented foray into economic literature produced no useful insight, as all I could find was inseparable from money and market economics.

3.2 New footing: Latourian Actor-Network

During this early migratory phase of my investigations, I stumbled upon Actor-Network Theory, abbreviated as ANT, by pure chance as described in Chapter 2.3. While in network-related articles and books ANT and its principal instigator Bruno Latour had been referred to several times, the perspective was largely dismissed or circumvented by the authors and accordingly by myself as well. Nevertheless, some aspects of ANT had undoubtedly been taken up by archaeology, which became clear as I delved deeper into the theoretical realm.

According to Latour (2005), Actor-Network represents a critique of traditional Durkheimian sociology and its offshoot, critical sociology, which are seen as products of modernity. Latour's earlier work has extensively focused on the critique of modernity, consisting of such ideas as the pre-eminence of humans, the passiveness of objects, and a strict division between culture and nature (see Latour, 1993). On discussing ANT with my colleagues and reviewing the literature, I found it to be largely misunderstood, from implementation to naming. Tellingly, even the main composers and developers of the idea have criticized the terms actor, network, and theory, and also the hyphens in between (see Latour, 1999; 2005: 9). Indeed, Latour's first pick for the name was *sociologie de la traduction*, sociology of translation. Eventually, after profound confusion, I found this name profoundly clarifying.

In its sociological form the theory can be seen as less of an applicable theory and more as a methodology, or the first attempt at completely restructuring sociology. Whereas Actor-Network Theory has been translated into archaeology mostly as the agency of objects (see e.g. Olsen, Shanks, Webmoor, & Witmore, 2012), the sociological ANT is more overarching, focusing on reorganizing the concept of *social* and, with it, *society* as a whole (see Latour, 2005). "Social" is neither a description (e.g. "socio-economic") nor a result (e.g. "social explanation"), but a movement of action passing between mediators. As Latour forcefully states, 'there is no society, no social realm, and no social ties', but rather there are 'traceable associations' resulting from actions that have been translated through

different mediators (Latour, 2005: 108). In other words, society even in modern state context is always an abstraction.

Translation, the key term in understanding Actor-Networks, occurs when action passes between mediators, or actors, who may consist of both living and non-living beings. Associations between physical and metaphysical things arrange into a bafflingly complex assemblage, in which the traceable elements may be observed to undergo significant changes during their movement. Hence ANT is called a network theory, although Latour has argued that the term network is inapplicable to ANT in its current Digital Age form, as network implies 'transportation without deformation' (Latour, 1999).

I apologize to all readers who take this as gibberish. The language of Actor-Network Theory has been molded through exchanges between French and English-speaking academia. I suspect that the clarity of ANT suffers from its own (linguistic) translations, with nuances in terminology transforming as definitions are passed between languages. The main point is that society exists only as a simplified umbrella-term for actions and participants, without existing in the world, except in modernity as a cultural construct.

3.3 Digging deeper into Modes of Existence

Bruno Latour has expanded his methodology far beyond Actor-Networks. In fact, he has acknowledged that ANT is mostly useful in tracing and describing the movement of associations, but the values that comprise the movement remain untenable (see Latour, 2013: 35–37, 63–64). Accepting the arguable descriptive flaw of ANT, he proceeded with a project that eventually became the book 'An Inquiry into Modes of Existence' (Latour, 2013). The purpose of the project was encapsulated by the subtitle, 'Anthropology of the Moderns', meaning the people ascribing to the Western worldview and its enlightenment heritage.

The Modes of Existence methodology constitutes a fragmentation of the simplified term "society" into, at least, 15 distinct relational modes, each representing its own reality. Actor-Network in Modes of Existence methodology is used as a tool for tracing relations that run throughout and between the modes in networks comprised of transforming actions which connect actors (Latour, 2013: 28–46, 95). The network exists recognizably within the frames of its own mode. However, instead of the networks themselves, it is the crossings between different modes, where actions need to be translated the most, that interest Latour, because these provide contrasts that reveal the value systems of the respective modes.

Table 1. My summary of Latour's formulation of modern society (based on Latour, 2013: 488–489), with thematic groupings and verbs of action.

Theme	Mode of existence	'How to…'
1st	Reproduction	'…extend in time.'
Processes	Metamorphosis	'…change.'
	Habit	'…be uninterrupted.'
2nd	Technology	'…invent.'
Science	Fiction	'…imagine.'
	Reference	'…know.'
3rd	Politics	'…cyclically regroup.'
Institutions	Law	'…maintain order.'
	Religion	'…have faith.'
4th	Attachment	'…pull towards.'
Economy	Organization	'…arrange.'
	Morality	'…navigate scruples.'
5th	Network	'…pass through modes.'
Metaphysics	Preposition	'…guide thinking.'
	Double Click	'…recognize assumptions.'

Modes of existence constitute the collective, which is a term more encompassing than society, including living and non-living as well as purely metaphysical entities, all participating in actions. Modes include commonly expressed aspects, such as law, religion, and politics (see Table 1). These modes have been placed by the moderns in delimited domains, *domain* implying a separate categorization of an institution within society that moderns insist stands on its own. This is evident in statements such as "courts are above politics"—even though politicians make the laws—or "separation between church and state". Yet the Actor-Networks in which law, religion and politics are carried out intertwine so that they encroach on other modes. This is because passing between them are translated actions. By contrasting modes with each other, their relativity becomes observable (e.g. Latour, 2013: 62). For example, while actions regularly pass between law, religion, and politics, all require different methods of verification, or felicity conditions, according to which the actions need to be translated to be deemed successful (Latour, 2013: 53–56).

To be clear, such modes are not merely the commonly defined institutions which share their names, but frames in which actions are undertaken. Latour introduces several unconventional modes, arranged into thematic groups based on their subject-object relations. Law, religion, and politics constitute only the most visible grouping, institutions. Others include for instance preposition, reference, reproduction, attachment, and metamorphosis. Each mode means a distinct applicable action. For example in archaeological research, *prepositioning* by *referencing* to *fiction* (see Latour, 2013: 250–251) entails imagining the past as it were lived, based on the evidence at hand. Thus, the Modes of Existence methodology offers aspects that are projectable, with due caution, to prehistory. Most importantly, it offers self-reflection to an archaeologist: either we acknowledge that we ultimately dwell in fiction, which is where all hypotheses are formulated, or we simply assume to know. Assumption, meaning most of all the use of common sense, is referred to by Latour by the computer metaphor of Double Click, a process meant to mask a complex set of code activation as a simple two-step cause-and-effect. In general, cause-and-effect, while often a tempting notion, serves to hide many branching actions, allowing simplistic and often one-sided interpretation.

A Modes of Existence perspective also affects ethnographies, which archaeological interpretation frequently relies on, especially by bringing to light several important biases which Western ethnographers have carried with them to the field. The most important biases are those of society (for Latour, the narrow Durkheimian definition of the amalgamation of two modes: *reproduction* and *organization*; or the continuation of arrangements) and economy.

Economy in particular in Latourian thinking becomes a question of how movements of attachments—meaning things that attract others to engage with them, such as material goods—are arranged in an unreachably optimal way. Thus, in the same way as society, instead of being an entity or a form of existence, economy should be understood as a simplified linguistic aid useful in conceptualizing an elaborate network of translated actions. Instead of economic value existing as fact, value is determined through metaphysical means. Instead of a science, which can be applied to humanity everywhere, modern economics is, in Latour's words, 'more like spiritual exercises, like the challenging discipline of yoga' (Latour, 2013: 465).

Merriam-Webster's first definition for the verb *discipline* is 'the practice of training people to obey rules or a code of behavior, using punishment to correct disobedience'. This definition, while not the only one, fits well with the teaching of, at least, bachelor level economics in the UK and US, which, in the wake of the

2007–2009 Great Recession (see e.g. Stiglitz, 2013), has been criticized by students as passing down the same uncritical codified behavior that led to the crisis (see Earle, Moran & Ward-Perkins, 2017). Thus, applying a Modes of Existence perspective to a pre-marketplace context requires us to seriously rethink the role played by the economy, a cultural concept that is difficult to separate from its modern use.

3.4 Forgoing economy for relations

Economy originally meant the managing of a household, or housekeeping. As an adjective it means frugal. In contrast, the economy as its own entity, a 'distinct system with its internal logic' (Earle et al., 2017: 14), is a rather recent invention. It was only made popular by the neoclassical school of economics. The rhetoric of the "domain of economy" began to permeate politics and eventually everyday life only after the Second World War, which had given economists an arena to showcase their talents for mathematical strategic resource allocation (Earle et al., 2017: 14–18). The "economization of the world" affected archaeological theory as well. The influence of the neoclassical school of economics is reflected especially in processualist archaeology, which emerged in the 1950s, instigated by modern systems theories in general (see Urban & Schortman, 2019: 73–84).

When it was first imagined as the study of an entity, economics was intended to describe the contemporary present state of affairs, not the past. In fact, economic theory is partly based on unfounded assumptions. Anthropologist David Graeber levelled a stirring critique of Adam Smith's account of barter. Smith, often cited as the founder of economics, understood barter as, what might be called, the primordial economic system from which the market economy grew (Graeber, 2011: 21–45). Money, in Smith's account, was created because barter, meaning the exchange of objects of similar value, which according to Smith was the economic principle of the pre-monetary societies, was too inflexible to work efficiently. Barter is in essence trade in the absence of money, and it has been applied to many archaeological interpretations as an economic system. To barter, then, is to use money as a value system, but not as exchanged currency. Graeber asked, how could this trade-minus-money system have emerged prior to currency? And with this question, the whole concept of barter becomes suspect in the context of the current study.

> Tellingly, this story played a crucial role not only in founding the discipline of economics, but in the very idea that there was something called "the economy," which operated by its own rules, separate from moral or political life, that economists could take as their field of study. (Graeber, 2011: 27)

Graeber cited the anthropologist Caroline Humphrey, who posited that a pre-monetary barter-based economy has never been observed in ethnographic studies (Humphrey, 1985). Instead, Graeber argued, there are three main forms of transactions—communism (transaction without expected reciprocity), exchange (reciprocal transaction), and hierarchy (obligational transaction) —and barter is the most impersonal form of exchange, which was rarely, if ever, systematically practiced (Graber, 2011: 94–126).

But how can we explain the movement of objects in prehistory if not as barter? We know, inherently, that in our studied time and place there was some form of "trade", long before currency. At least objects moved throughout Fennoscandia, some from faraway places. Trade is an oft-invoked explanation for this, but the concept is muddled in assumptions, mostly about barter. For instance, the late archaeologist Matti Huurre, in one of his general prehistoric narratives of Finland, pondered Stone Age material circulation:

> Even though self-sufficiency was vital in Stone Age circumstances, **exchange** developed early on between different areas. Its character is difficult to know for certain. *Perhaps the question was not yet about actual trade*, but **gift-exchange** between groups, which has with many natural folk attained complicated and strictly scripted forms. On the other hand, although one might thusly explain the movement of wares from place to place, at least regarding a few products it feels justified to speak of **trade**. […] …It is presumed that on the shores of Lake Onega, in conjunction with quarries, there was actual stone tool industry and green slate was not released on the **markets** other than in highly processed form. […] Trade was undoubtedly **barter**. (Huurre, 1979: 56, my translation and emphases)

Accounts such as this are difficult to disentangle from their deep roots in modern economics. Barter continues to be invoked in a more recent Finnish prehistoric narrative of the Stone Age (see Halinen, 2015: 49), also as undefined trade that existed alongside gift-exchange in the Bronze Age (see Lavento, 2015: 172–179), and all the way to the near-mercantilist yet pre-monetary narrative of roaming maritime traders of the Late Iron Age (Raninen, & Wessman, 2015: 348–349).

Barter and trade, which often seem practically synonymous, are thus deeply ingrained in Finnish archaeology. This is so, despite there having been, to my knowledge, no consideration of contemporary attitudes toward debt, which is the foundation of trade (see Graeber, 2011). Both Latour's and Graeber's critiques suggest that we ought to seek new foundations, besides barter or the domain of economy for the movement of materiality.

It should be noted that not all anthropologists share Graeber's critical view of the theory of barter. For instance, the archaeologist Charles Stanish criticized Graeber's omission of Malinowski's account of barter alongside the Kula Circle, in which objects of similar value changed hands (Stanish, 2017: 284–285). Instead he insists that barter is fundamental in explaining the evolution of so-called *complex stateless societies* and economies (Stanish, 2017: 93–95). On the other hand, Stanish bases his generalized account on the insistence that 'reciprocity is the fundamental socioeconomic mechanism of co-operation in virtually all societies' (Stanish, 2017: 80). In doing so, Stanish himself disregards the real possibility that reciprocity is not in fact universal (see Bird-David, 1990; Graeber, 2011: 91–94).

There is a clear continuation from Bronisław Malinowski's (1922) description of the Trobriandian Kula Circle, a form of cyclical exchange, to Marcel Mauss's (1954) seminal essays on the concept of the reciprocal gift. Mauss incorporated Malinowski's field work to his analysis and supplemented it with a wide array of ethnographic and historical examples, laying the foundation for a rivaling account of a primordial barter system: the gift exchange economy. Marshall Sahlins's (1972) formulation of the 'original affluent society', which showed that prehistoric societies were not scraping by on a bare minimum, relied heavily on Mauss's gift exchange theory, while Graeber, a student of Sahlins, carried on his teacher's legacy.

It is easy to see the continuation from Mauss to Sahlins to Graeber. The problem for us is that Mauss, a student and nephew of Émile Durkheim, understandably followed the Durkheimian sociological approach, as did Malinowski. Thus, *society*, *economy*, and *social explanation* feature prominently in both Malinowski's and Mauss's seminal formulations. Now, based on the Latourian approach adapted for this study, these three concepts are oversimplifications which have little bearing on reality. Does this mean that the two theoretical paths of Latour and Graeber are completely incompatible? Fortunately, Graeber, while not completely abandoning society as an analytical group, clearly struggles with the concept, as we shall see.

But first, in collating the long-winded argument, we have to account for a scenario where people did not by default barter, trade, or *exchange* things. Instead

they simply gave when they had something to give or asked for something when they wanted it. Expecting return from such an "investment" has had to have been invented and maintained, so I concede that reciprocity has to be observed in the archaeological record, somehow, before its existence as a common logic can be asserted. As of yet, there is no reason to presume that the subject people had to compete for nourishment (see Chapter 4.2), nor is there any evidence to suggest that property rights were the default condition.

We can reduce the distance between objects and subjects by considering whether want, or in Latour's term, attachment, emanates only from within, or perhaps also from without or from the thing itself. If someone wants an object that you have, could it be that the object itself wants to change hands? Who are you to argue with it? "What's in it for me?", in this setting, is a question asked only if, first, you want nothing further to do with the other person, and second, if you are willing to harm your own reputation, and third, if you do not understand what being cut off from transactions means to you. You would be known as a difficult person, who dares to block the circulation of things. If enough people were to become difficult, the movement of objects would stop, and so would everyone's chances of engaging with the world through items. Why diminish your own agency by accumulating objects and not letting them go when you could instead be multiplying yourself by giving and receiving?

Material movements, then, are not solely or even by default reflections of the desire to gain items but are about engagement with both people and things. Being indebted to someone is a way to maintain a relationship (see Graeber, 2011). Giving someone a gift means that a part of you continues to exist within the gift and, along with it, the receiver. In this sense, a good life, therefore, is giving, as the act expands even your person.

Yet there is another way to maintain the flow, besides everyone complying. Should there be someone so "ignorant and rude" as to try to block the flow, stealing is always an option. Gift and theft are from the archaeological perspective nearly the same. The difference is that theft relates to violence. Thus, a happy-go-lucky cycle of giving and receiving cannot be imagined without arguments, feuds, injury, pain, and suffering as well. Attachments require sacrifice.

With this formulation in mind, let us see if we can implement it to any effect. I hope to be excused should I happen to unconsciously continue invoking the economy as a living entity. A child of the 1980s, with two recessions under my belt, and one ongoing, my indoctrination to the ideology of the economy has been inescapable my whole life, so it is bound to seep through in my rhetoric. First, as

per our theoretical framework, we will have to observe the deficiencies of the second abstraction, society, and try to formulate a more robust definition for collectives.

3.5 Culture and awkwardness of society

In the Latourian sense, society as well as economy are practically non-existent. Mere shorthand, they are symbols for understanding associations that are too complex to easily define. Nevertheless, anthropology is saturated with both terms. In order to understand the concept of society and what it entails in a given situation, let us begin with a basic and fairly recent definition, which derives from Émile Durkheim's formulation.

> A society is a system of interrelationships which connects individuals together. Britain, France and the United States are societies in this sense. They include millions of people. Others, like the earliest hunting and gathering societies, can be as small as thirty or forty people. All societies are united by the fact that their members are organized in structured social relationships according to a unique culture. No cultures could exist without societies. But equally, no society could exist without culture. (Giddens, 2001: 22).

Here, the sociologist Anthony Giddens provides us with a variant of the age-old question: which came first, the chicken or the egg? To us, following what we have discussed before, the question between society and culture is a question that is just as puzzling as the more popular one. A hint: the first shelled eggs precede the domestication of the red junglefowl by more than 320 million years, so the answer to that question is a resounding egg.

First, a short detour on the concept of culture as I understand it. I see culture as both cognitive and material. Sometimes understood as separate definitions of culture, both were criticized by anthropologist Clifford Geertz, stating that culture is not solely in the mind nor is it solely made of objects (Geertz, 1973/2017: 12–13). Thus, we can deduce that following Geertz's argument, and considering the archaeological material perspective, culture is both metaphysical and physical. Geertz's own definition can be summarized as shared symbolic action forming a context (Geertz, 1973/2017: 11–15).

Despite Geertz's apprehension of culture as cognitive, and despite his critique of such a cognitive anthropological formulation, I am inclined to agree with exactly this latter view, but in ways which Geertz might have accepted. The bridge between

the two, Geertz and cognitive anthropology, in my mind, is found in cultural model theory. Cultural models, assembled from the sharing of individual personal experiences and understanding, which are sometimes designated as *schemas*, represent collective understanding and ways of thinking and interpreting (e.g. Shore, 1996: 44–54; Quinn, 2005; D'Andrade, 2005). In this cognitive perspective, a cultural context is made up of many related shared cultural models relating to the circumstances. Individuals may hold many personal schemas, which they decide not to share. Instead, cultural models only constitute notions that are shared. This functions both as a context and an abstract entity. Culture influences personal schemas, as with the concepts of the norm, honorable behavior, and immorality, all which affect conduct without creating unbreakable boundaries. However, culture is also being constantly reformulated according to the flow of the schemas individuals choose to share. Culture, overall, is then an amalgam of often contradictory shared cultural models and materiality infused with meaning. Applying ANT and granting agency to objects, in a similar way that culture itself has agency on people, our definition of culture as a cognitive, material, and interactional context is complete.

But what about society? Giddens highlighted two contrasting levels of society, that of 'Britain, France and the United States' and those of 'hunting and gathering'. The latter are 'as small as thirty or forty people.' Presumably, there is every possible variation in between. As one definition apologetically states, on the smallest scale, 'the term is often used interchangeably, and confusedly, with community' (Clarke, 1996). However, society is usually defined with the former large-scale example in mind, as Graeber explains:

> Let me return again to that word, "society." The reason that it seems like such a simple, self-evident concept is because we mostly use it as a synonym for "nation." After all, when Americans speak of paying their debt to society, they are not thinking of their responsibilities to people who live in Sweden. It's only the modern state, with its elaborate border controls and social policies, that enables us to imagine "society" in this way, as a single bounded entity. This is why projecting that notion backwards into Vedic or Medieval times will always be deceptive, even though we don't really have another word. (Graeber, 2011: 69)

This leaves us with a true paradox. A society is a nation state, but also as small as thirty people, say, an extended family. Yet, an extended family within a nation state is not a society. If we bring this definition to archaeology, we can state that, first, a single dwelling site could constitute a society; second, an assemblage of multiple

contemporary dwelling sites could constitute a society; third, all contemporary archaeology within some defined boundary, e.g. a later nation state, could constitute a society.

Society in the third sense has contributed much to Finnish archaeology. With this definition, Matti Huurre managed to summarize the society of Finland's Stone Age in two pages (see Huurre, 1979: 67–69). A more nuanced summary is presented in the more recent prehistory, again in two pages, where the word society is prudently omitted (see Halinen, 2015: 50–51). The term was replaced by the plural *communities*, which allows more variation and also a closer ontological distance to archaeological remains. That community has supplanted society in Finnish language archaeology is not a surprise. The practically synonymous Finnish word for the English word society is *yhteiskunta*, which is the composite of the words *common* (or *shared*) and *municipality*. It is understandable why the word's projection onto prehistory is rapidly fading. Nevertheless, even many Finnish archaeologists still use the English term.

As we have seen with the issue of scale, the term society is often refurbished with defined categories. Such categorizations allow more descriptive precision for the concept, but it frequently muddles things further. We already encountered the term complex stateless society, as used by Charles Stanish. He defined this as follows.

> I refer to the small-scale societies in which people build special places as a means of organizing their economic and cultural life as complex stateless ones. The key difference between stateless societies and complex ones is the degree to which people in the latter co-operate on a sustained basis with others who are distantly related or even not biologically related at all. Most hunter-gatherer bands—simple stateless societies—lived in groups that averaged approximately 25 people. Complex stateless societies supported substantially larger numbers of people who interacted with each other over long periods of time. While there is some evidence that hunter-forager bands were composed of some nonrelated people, complex stateless societies were characterized by numbers an order of magnitude larger. These large numbers created complex relationships between many people. (Stanish, 2017: 1)

Now, not to be too critical, this definition seems to bring three more problems alongside society. All three are dichotomies: small/large; simple/complex; stateless/state. This echoes the century-and-a-half old conceptualization of society by Herbert Spencer in the mid-19th century. He argued that societies are organic

and evolve from small to large, from simple to complex, and from many to few. In doing so, he quite openly projected aspects of the newly found process of biological evolution on human culture (McGee & Warms, 2012: 13–23). The justification for the dichotomy of society between simplicity and complexity is particularly jarring, when expounded in the parlance of the time.

> In complexity, our large civilized nations as much exceed the primitive savage ones, as a vertebrate animal does a zoophyte. And while in simple communities, as in simple creatures, the mutual dependence of parts is so slight, that subdivision or mutilation causes but little inconvenience; in complex communities as in complex creatures, you cannot remove or injure any considerable organ without producing great disturbance or death of the rest. (Spencer, [1860] cited by McGee & Warms, 2012: 17)

This biological analogy, although not explicitly propagated any longer, remains implicitly embedded in the conceptualization applied by Stanish (see Giddens, 1984: 163). So, instead of considering how such dichotomies necessarily classify some people as simple savages, on the axis unimportant/important, let us focus more on how Spencer's concept of irreducibility stands.

In the organic metaphor the head of a modern society is arguably the state government, which may variously represent despotic rulers or representative cabinets. According to Spencer, in complex organisms such as a 'large civilized nation', 'you cannot remove or injure any considerable organ without producing great disturbance or death of the rest.' Now, the longest period that a modern state has been without a government is 541 days. This was Belgium in 2010–2011, which is what Spencer would have probably considered a large, civilized nation. Instead of dying, the state and infrastructure persisted. Not only that, but there are also some indications that Belgium did somewhat better while decapitated than it normally would have (Graeber, 2018: 209; Albalate & Bel, 2020). Is Belgium that simple?

Stanish, in his definition, classifies stateless societies based on their simplicity and complexity. But this, as is clear in the previously cited definition, is actually a question of small and large, separated "by numbers an order of magnitude larger." In other words, complexity is about population size and density. Try as we might, in the context of this dissertation, we lack the adequate means to measure such attributes in prehistory (see Chapter 9.4).

Simplicity and complexity are not the only problematic, yet frequently evoked, categories of society. The term *stateless society* brings us another one. In the words

of sociologist Pierre Clastres, 'what the statement says, in fact, is that primitive societies are missing something – the State – that is essential to them, as it is to any other society: our own, for instance' (Clastres, 1989: 189).

Society as a concept, then, brings us inevitably back to the state. Its application as a separate entity, while not completely impossible, is, considering the preceding discussion, often implausible and awkward. It is even more awkward in places in time where the state has never been. Since we are dealing with such a prehistory where no evidence of state formation has come to light, we might be better off not to project onto it a form of organization, whose most prominent effect would be the constant reminder of the absence of our own institutions (see also Chapter 9.2).

So, returning to the chicken-and-egg of society and culture, can culture survive without having ever been in contact with society? I would argue that culture, in the previously defined sense, lives on robustly in the past, for at least as long back in time as tools have been in use, and probably even longer. This is up to the paleoanthropologists. While society is arguably a colossal anachronism when applied to, say, quartz discards from a Mesolithic site, culture maintains its relevance. Even those scraps, irrelevant as they are to many people, cannot be denied their claim as cultural heritage.

3.6 Community as foundation for collectives

In the absence of society, how can we define archaeological collectives? One answer lies in starting not from the top, but from the bottom. For the purposes of this study, as we are studying human activity, we will begin with the individual. This person is admittedly not a singular entity. She sheds stone discards, starts fires, and burns food waste. Her actions percolate. But archaeologically she is untraceable. Even her dead body lying in her grave is distant from the actions she performed.

We know she interacted, and we can safely assume, without disrespecting our subject, that she belonged to a group. Among the common groups are families. Yet, families differ culturally so much, that the concept of family offers little in the way of concrete and shared definitions for analysis. After all, as adult couples may be childless, so may siblings be orphans, so two families may exist as mirror images. So, we have to look for a higher plane of group abstraction; one that allows a shared definition.

You may recall those groups of 30 or so people, that were previously designated as "societies." You may also recall the confusion arising from such

small-scale designations. One may question the reason why these were ever designated as societies, when clearly, as Clarke seemed to point out, what is actually being described are communities (see also Bird-David, 2018).

The concept began resonating with me after reading archaeologist Peter Holmblad's doctoral dissertation, *Coastal Communities on the Move* (Holmblad, 2010). My attention was initially drawn to it because of the geographic and temporal connection to my own study. Holmblad's study took place on the Finnish Kvarken coast in the Bronze and Early Iron Age. But also, he focused his perspective on archaeological site assemblages, in a similar fashion to what I had in mind.

Holmblad defined communities as 'small local societies.' This definition worked rather well in the context of society as 'human social organisation on higher levels of abstraction' (Holmblad, 2010: 14). However, these concepts do not serve us particularly well. To us, with the Latourian framework in the toolbox, such a definition would encompass practically everything, even with humans prefixed into it.

More recently, archaeologists Patricia Urban and Edward Schortman, while discussing group identities in the context of the Pre-Colonial Naco Valley, offered a suggestion along with a useful definition:

> One concept you may want to take with you is that of **community**. A community is a group of individuals united by locality and common practices[…] The use of community avoids some of the problems embedded in ethnicity such as figuring out language and foodways and deciding whether those practices were charged with ethnic meanings by those who engaged in them. Still, do not be lulled into the notion that within communities all people lived lives of quiet conformity. […] Some [referring to the inhabitants of a specific Naco Valley site] resided in relatively grand accommodations, bespeaking a level of power not matched by those occupying humble abodes at that settlement and its near neighbors. (Urban & Schortman, 2019: 189)

To this valuable insight I may add that communities are not made up only of humans. Instead, they are collections of people, structures and objects, ideas, and other living beings whether plant life or animals. The inclusion of animals as integral parts of society, and also communities, is not a novel concept (see e.g. Armstrong Oma, 2010). Even the legendary, and old-school, Finnish folklorist and professor of archaeology, the late Unto Salo, as his final Parthian shot, advocated the inclusion of animals in the collective (Salo, 2019: 164).

Because of its delimited inclusiveness, the term community resonates particularly well with archaeology. I began using the slightly differing concept of local community as a way of passage from archaeological sites to lived experiences. This is admittedly rather naïve, but it has served me well as a theoretical trajectory. Instead of society, a community—whether sedentary or mobile or semi-in-between—occupies a place in time and, I would argue, exists empirically within interactions in the physical world. Communities are assortments of habits. The difficulty in defining the term originates in its relativity. It is defined almost exclusively in the negative, not as "how we define ourselves", but as "how we differentiate ourselves from others". But such relative definition, it seems, has rarely been propagated by sociologists and anthropologists relating to the communal scale (see e.g. Kolb & Stead, 1997; Yaeger & Canuto, 2000).

Ploughing through definitions of community, as collated in accounts by archaeologists Michael Kolb and James Stead (1997) and Jason Yaeger and Marcello Canuto (2000), reveals how such definitions fail to account for differentiation. 'Relatively static, conservative, closed, and homogenous', 'maintained by residential proximity, shared normative culture, and the daily experiences common to its members' represents one distillation of earlier definitions collated by Yaeger and Canuto (2000: 3). Other definitions include 'the number of people who normally reside in a face-to-face association' (Murdock & Wilson, 1972, cited by Kolb & Stead, 1997: 611). Kolb and Stead themselves propose community as 'a minimal, spatially defined locus of human activity that incorporates social reproduction, subsistence production, and self-identification' (Kolb & Stead, 1997: 611).

Self-identification, 'the creation and maintenance of local identity' (Kolb & Stead, 1997: 611), it remains unsaid, requires contrasts to others. Whether communities in general are "static, closed, and homogenous", or even arguably whether they are "conservative", depends on the definition. If we understand communities as being relational, they are conceptually pluralistic and in constant motion, because they react with their surroundings. Canuto and Yeager's proposition that communities are 'constituted by interpersonal interactions and negotiations that are rendered salient by a shared framework of expectations' and 'communities develop and exist within broader social and political contexts' (Canuto & Yeager, 2012) still do not explain why boundaries form between communities.

The so-called Dunbar's number offers one explanation. According to the hypothesis, as part of the Social Brain Hypothesis, the average number of 'weak

social ties' that individuals can maintain is about 150. This follows a generalized pattern of the quantity and quality of social ties, 5–15–50–150, beginning from the closest intimates to dear friends to frequent acquaintances to familiar faces (e.g. Sutcliffe, Dunbar, Binder, & Arrow, 2012). Dunbar's number, arguably too neat and universal, is applicable only when wide error margins for individual variance are accounted for. Whether cultural differences cause large deviations from the hypothetical rule remains to be seen. We can still apply Dunbar's number as an in-the-ball-park evaluation for the limited extent of associations between individuals. This is another reason why society is necessarily a crude abstraction, a rhetorical tool. Society is often conceptualized as collectives whose numbers far exceed the number of associations individuals can cope with.

It is generally acknowledged that the number of people we can empathize with at one time is limited. When exceeding the limit, we tend to forget about someone else, someone we have not thought about for a long time. Because of this, there have recently been calls for the expulsion of empathy from various forms of interaction. Among the most prominent proponents of this theory is the philosopher Jesse Prinz, whose ideas (see e.g. Printz, 2011a; 2011b) inspired the psychologist Paul Bloom to write the widely popular book *Against Empathy* (Bloom, 2016).

Empathy, according to its detractors, is subjective, never objective, and as such it is ineffective and arguably harmful, even in moral activities that espouse empathy, such as humanitarian aid work. Scientific rationalism, the argument goes, should be the only logical and objective foundation for actions ranging from morality to justice to social policy (see Bloom, 2016). This argument relies on a narrow sentimentalist definition for empathy: the feeling or mirroring of other people's feelings (see Coplan, 2011). This definition is narrower and more constricted than most other definitions (Passos-Ferreira, 2015; Fagiano, 2016), so the application of empathy critique to general discourse may actually be more subjective—and harmful—than intended.

However, and whatever the definition, empathy is what humans have become attuned to (see e.g. Zahn-Waxler & Radke-Yarrow, 1990; Eisenberg, 2007; Singer, 2007). This is at least partly because empathy has a longer history than humanity. Although it has taken a long time to be acknowledged by the "rational sciences," empathy has been observed in interactions of not only primates, but also of more distant relatives to humans, such as elephants and mice (e.g. Safina, 2015; 58–65; de Waal, 2019: 79–120). It turns out that understanding the emotions of others is not exceptional to humans.

Empathy in its pluralistic sense (see Fagiano, 2016) is at the most basic level the interpretation of the emotions of others. I would argue that it is an ability that makes communities possible. It may also serve as a prerequisite for the urge to share (see Chapter 3.4). In a controlled experiment with chimpanzees, described by Yamamoto, Humle, & Tanaka (2012), a mother-child pair were separated into adjacent rooms connected by an open panel. One room contained a puzzle with a reward, while the other room contained an assemblage of tools, one of which was needed to solve the puzzle. The chimp with the tools would gladly hand the other one tools through the panel, even though there was no reward involved for the tool-bearing chimp, but only for the problem solver. In other words, there was no reciprocity. Such behavior, i.e. understanding the goals of the other and the willingness to help, requires empathy (Yamamoto et al., 2012). Although in this instance empathy was observed between close kin, ethologist Frans de Waal has described many similar occasions where kinship has not been a factor (de Waal, 2019: 114–120).

This brings us back to the problematic assumption of the universality of reciprocal exchange (see Chapter 3.4). If helping and sharing is innate in a variety of empathetic mammals that live in packs or herds, this would suggest that the motive for both the sharing of resources and the formation of communities resides in the ability to empathize. Being able to relate to others, and their needs and wants, makes cooperation a moral imperative. It is common in immediate interaction that not being able to offer assistance brings forth an emotional response, the feeling of uselessness or scruples. Assisting, on the other hand, projects a positive physical feeling. It seems, with the rising tide of ethological observations that such a feeling is by-and-large not a cultural construct, but more biologically widespread than say barter, economics, or the conception of society.

While communities, I would argue, are maintained through the ability to empathize, the limits of empathy also form boundaries between such associations (see e.g. Fuchs, 2019). We already encountered a similar limit, as argued by the proponents of the Dunbar's Number. Combining the two perspectives, the Social Brain Hypothesis and the pluralistic view of empathy, gives us more understanding of why limits between associations form. These personal limits give communities and societies their own differing ontologies. While, communities exist within the boundaries of empathy, societies sprawl beyond its reach.

Another reason to favor communities instead of society as collectives is, as I mentioned before, that communities are physically grounded. Imagine taking part in a local celebration either in an urban neighborhood or a rural village. Asking the

participants, “where is this community”, would elicit a response along the lines of “look around you dummy, it is here.” Now, ask “where is this society,” and the answer would be “it is primarily somewhere else, out there, around us.” Town halls, grocery stores, cafes, bars, and playgrounds are places where communities can actually be experienced, while society is always out of reach, whether grasped at by a leading politician, a major media executive or a pedantic sociologist. The physical connection gives the concept of community special relevance in archaeology.

In the terminology used in this dissertation, community and local community are applied in slightly varying ways. I use local community as a way of assembling geographically delimited areas of activity, that are larger than individual archaeological sites. So, whereas the term community could be taken as consisting in time of an individual dwelling site and its immediate surroundings, the term local community may include several nearby dwelling sites and places of labor and subsistence, i.e. hunting grounds, as well as ceremonial places. The reason for such geographical delimiting is admittedly to ease the analysis, but also, as will be argued in Chapter 4.2, because certain forces of attraction and agency are embedded in the local geography.

3.7 Entanglement of practical actions into ideology

Before proceeding to the physical world, we need to wrap up some loose ends. By returning back to network theory, we can provide the study with some reference of how actions are performed and how they affect. We will first of all consider the impact of the Theory of Entanglement, articulated by archaeologist Ian Hodder, on Modes-of-Existence thought.

In his book, *Entangled*, Hodder (2012) emphasized the dependencies that entrap objects, immaterial things, and organisms. Through entanglement, where actions unintentionally form the foundations for subsequent actions, most often with no preplanned control over dependencies that arise from such foundations, technology itself becomes a constant management of problems arising from dependence. Suddenly inaccessible resources produce vacancies which cannot simply be disregarded, since their dependencies extend farther than the resource and the refined products themselves. The perspective of a lack of control over dependencies gives the Theory of Entanglement its unique pertinence.

In my mind there is no practical difference between Entanglement and ANT, besides the former’s emphasis on dependencies. Hodder himself criticized ANT

because in his view, ANT does not account for object-to-object relationships, but merely actions that occur between humans and objects. Thus, the argument goes, ANT does not acknowledge the natural world, such as the geothermal interactions within volcanos, which are—or at least have been—out of reach for human agency. This, to me, is a misrepresentation of Latour's argument. Some blame surely rests on Latour's writing style. But in light of my readings, I cannot account for statements such as '[…] in Latour's analyses things are always already caught up in networks of humans and non-humans and the object nature of things separate from society is not a key part of the analysis' (Hodder, 2012: 94). This seems to be just a misunderstanding. Since Latour's theory is based on the deconstruction of society, and the unravelling of the society vs. nature division, it is arguable whether Hodder's claim has any basis (see Latour, 2005: 109–120). What Latour means is that associations, i.e. social ties, are not exceptional to humans, and occur also between the supposedly inanimate.

The reason for my critique of Hodder's critique of Latour is not to dismiss Entanglement, but just the opposite. Mending this, what I see as a misunderstanding, makes the pieces fit perfectly together. Hodder's theory provides an elegant formulation of technological progress. It can be easy to mischaracterize Actor-Networks as rational formulations, where the goal of associations is toward efficiency. But considering how these associations were formed in the first place through entangled processes diminishes the urge to seek an evolutionary path for a given network. This notion redefines technology as something messier than the moralistic pursuit of efficiency, which is the way it is often conceptualized. Instead, technology is mostly the management of entropy in the form of unforeseen dependencies.

In the Entangled view, technological progress results from the perceived benefits granted by an invention and the new problems that it creates. The benefits discourage the abolishment of the new practices, while the problems they create motivate the search for ever newer inventions to solve them. Technology, then, keeps users hooked in the quest for the optimum, which, since new applications create new dependencies and new problems, can never be reached. In the process, as objects gain more and more relative context, their essence morphs.

This can be illustrated by considering a hypothetical narrative of the axe. Stone hand-axes allowed the chopping of wood and branches but were rough on the hands and their jagged irregular surface made them fragment easily. A wooden handle added leverage and softened the impact that the hands had to absorb, changing the essence of the hand-axe, making it relatively uncomfortable and inefficient to use.

With the axe-handle, deforestation became a possibility, opening an array of new ways of life, all depending on the axe. Polishing the blade gave it more durability, and thus unpolished axes became relatively fragile and inconvenient. Changing the material of the axe from stone to metal allowed a sharper blade, easing the effort by cutting deeper into wood, reducing the amount of work. This opened up more possibilities for the axe-wielder to perform during the day, forming new habits, which made it seemingly impossible to revert back to the stone axe. Today, cutting down a tree with a sharp steel axe instead of a chainsaw is seen as laborious, inefficient, and outdated. In fact, I would not be surprised if when a chainsaw malfunctions or runs out of fuel, most people would consider tree cutting practically impossible. Even the most enterprising lumberjack would call it a day should the handle of the trusty steel backup-axe break. There are not many people who would in that moment grab a few rocks, do some knapping, and, reverting to Paleolithic technology, start hacking down the thinnest trees.

Such an entanglement of actions is related to the concept of *chaîne opératoire*, or operational chains (see Delage, 2017). This outlook has been applied mostly for considering the different stages involved in manufacturing processes (see e.g. Inizan, Reduron-Ballinger, Roche, & Tixier, 1999: 16). Some operational chains are relatively simple, involving the gathering of raw materials, their modification into the desired item, the item's use, repair, and eventually its abandonment. We will look at particular operational chains in Chapter 6.1 (see also Paper III for more detailed analysis), when discussing and comparing prehistoric labor activities in Central Fennoscandia.

A theoretical concept related to operational chains, adopted to Fennoscandian archaeology from educational theory by archaeologists Fredrik Hallgren (2008) and Charlotte Damm (2012a), is *community of practice*. This is, instead of the group category discussed previously, a geographically unbounded network of learning and shared practices. Damm also used the phrase *learning network* synonymously with communities of practice (see Damm, 2012b). I implicitly applied a similar approach in Paper IV (see Chapter 6.2), where the networks of practices overrode geographic delimiters. In Paper V, community of practice is considered in reverse, with an emphasis on disassociations between geographies.

Returning to entanglements, one way to approach archaeological material identity from the angle of dependencies is to consider what Bruno Latour deemed *avidity*, or passionate interest. This derives from Gabriel Tarde's distinction between 'to be' and 'to have', or, as argued by Latour (2013: 424–425), identity and avidity. In archaeology, avidity could actually be a more materialistically

rooted concept than identity, although ‘to have left behind’ would be even more appropriate.

Avidity acts as a bridge between the material and the ideological, as the material realm is regarded as being populated by passionate interests. With the mode of attachment (see Table 1) possessing other actors, human and otherwise, even simply functional relations between a tool and a user are elaborated to involve more intricate associations. Issues of the material realm, such as technology and subsistence, are brought into the ideological realm. Further on, we will give more consideration to ideology as both a unifier and a divider.

4 The nature of Central Fennoscandia

Throughout the dissertation I use two geographic delimiters which may or may not be in common use. The first is the Bothnian Arc, or the concave coastal strip surrounding the Bothnian Bay. Why I chose to use this particular name is because it highlights the uniqueness of the region. I tend to use it when referring to an approximately 20 km deep coastal strip extending as a 500 km long arc from the reaches of Kokkola to Lövånger (see Fig. 1). When I refer to the Bothnian Arc in the context of previous time periods, it goes without saying that the strip is adjusted in relation to the contemporary coastline.

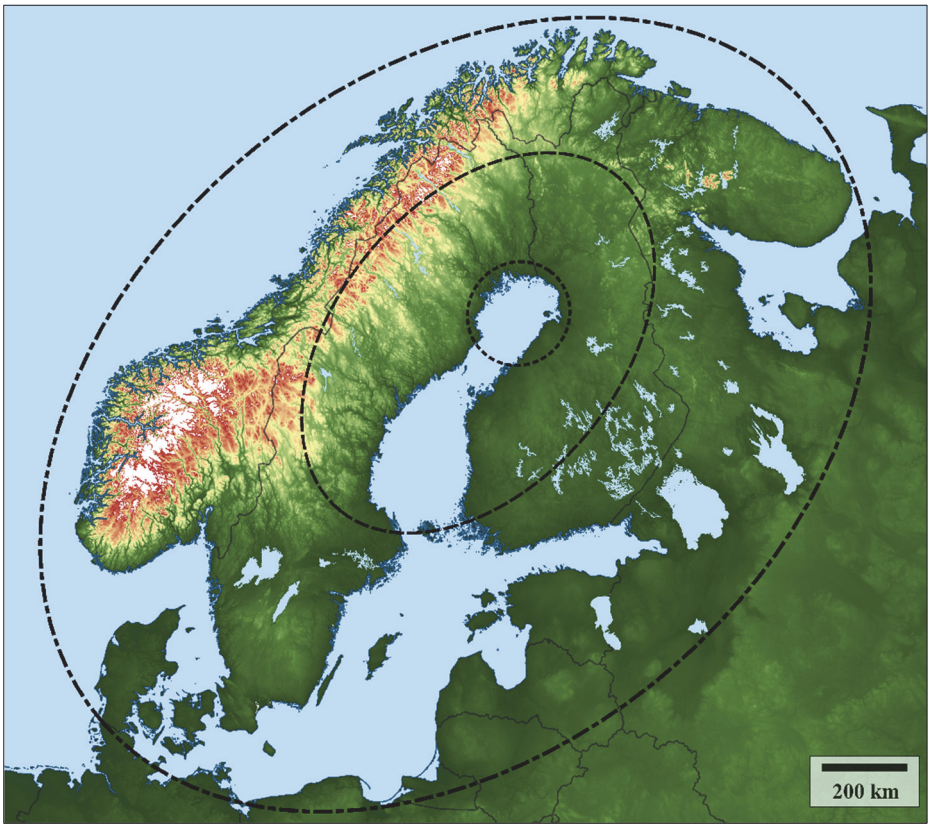

Fig. 1. Elevation map (DIVA-GIS) with the three regions of focus: Northern Europe (outer oval), Central Fennoscandia (inner oval), and the Bothnian Arc (small circle).

The other delimiter, Central Fennoscandia, is roughly the main watershed of the Gulf of Bothnia from the 60th up to the 67th parallel north, not including Inner Uppland, Åland, and Turku Archipelago. Fennoscandia itself is a commonly accepted geographical term, consisting of Norway, Sweden, Finland, Karelia, and the Kola Peninsula. I also use Northern Europe when referring to the regions of Denmark, Sweden, Norway, Finland, the Baltic countries, and Northwest Russia. Northeastern Europe would be the region between the White Sea and the Ural Mountain Range.

Geographically speaking, "local" is a problematic term, which is present even in the title of this dissertation. If we followed Actor-Networks pedantically, we would notice that the difference between local and global becomes too elusive to define (see e.g. Latour, 2005; 2018). I still insist on using "local" as a way of assigning communities a place. These places, I argue, can be roughly defined based on geography, and especially the acting entities embedded within geography.

4.1 The geography and past environment of Central Fennoscandia and the Bothnian Arc

The Bothnian Arc lies on the northernmost reaches of Europe's second mediterranean sea, the Baltic Sea. The North Baltic forks into two branches, the Gulf of Finland to the east and the Gulf of Bothnia to the north. The latter is larger, some 700 km long. The Gulf of Bothnia is composed of three parts. From the Åland islands, the sea opens into a 300 x 200 km basin called the Bothnian Sea. To the northeast, the sea is bordered by the Kvarken, a 170 x 90 km archipelagic strait, which leads into the 200 x 150 km Bothnian Bay, surrounded by the Bothnian Arc.

The geography of Central Fennoscandia is diverse, from the high mountain ranges in the northwest to the lowlands in the east. The lowlands, former seabed, are broken up by the uneven topography of the Finnish Lakeland further southeast. In the farther east and northeast, the hill country of Kainuu and Southern Lapland provide embedded passages for wide rivers flowing into the bay. In the west the mountains are preceded by up to 300 km wide uneven rolling slopes with vast river valleys breaching through to the central sea. More than 20 notable rivers flow into the Gulf of Bothnia. The rivers of the northwest descend in their valleys from the high snow-covered mountain range; the rivers of the southeast meander to the low-

lying coast from innumerable basins; in the northeast unstoppable swells massed from countless arctic streams course through their sculpted corridors (see Fig. 2).

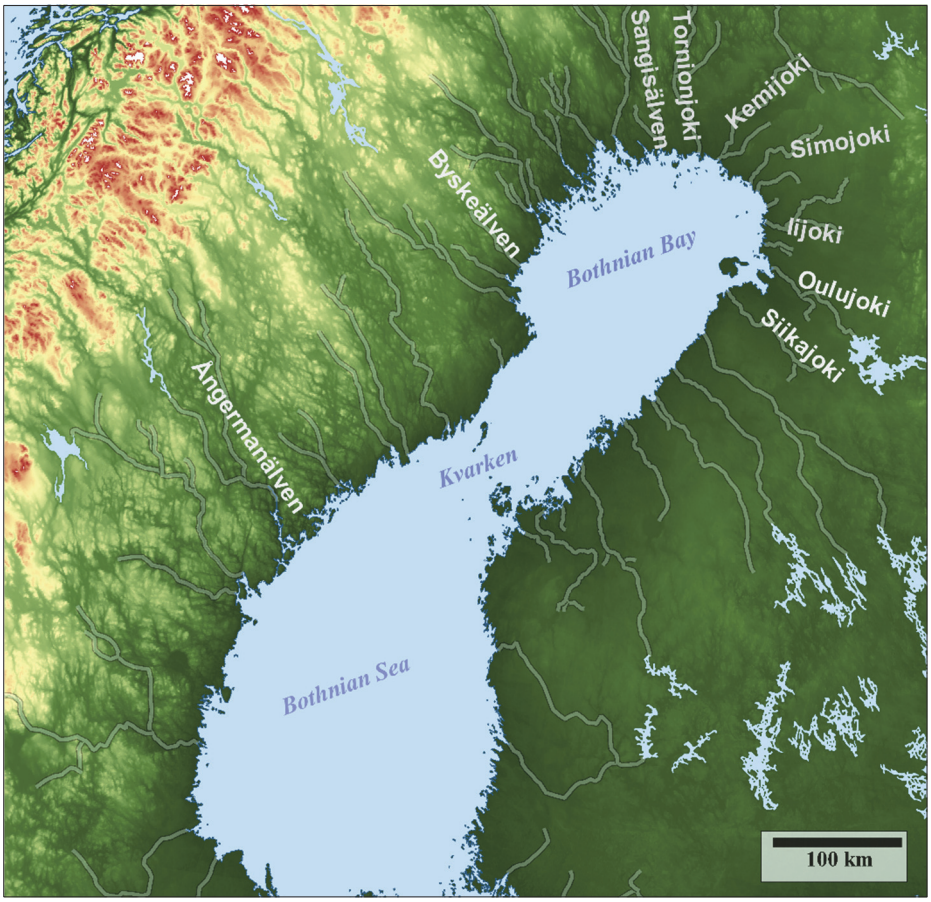

Fig. 2. Central Fennoscandia and the Gulf of Bothnia, with rivers mentioned in the text. Terrain elevation model (DIVA-GIS) and the myriad rivers flowing to the sea.

The land between rivers is covered mostly by taiga, or middle boreal forest. Evergreen pine (*Pinus*) and spruce (*Picea*) trees form the backbone of the flora, with cold-resistant deciduous trees, mainly birch (*Betula*) and alder (*Alnus*), also being frequent. In the coastal southern half of Central Fennoscandia, lime trees (*Tilia*) grow sparsely with pine and birch, while the very southern end is habitable for oak (*Quercus*), hazel (*Corylus*), and elm (*Ulmus*), which occur infrequently.

Research into the past climate suggests that a so-called Mid-Holocene Thermal Maximum lasted from approximately 6050 to 4000 BCE[1]. During this warm period, vegetation zones may have been transposed up to 300 km north of their current state (e.g. Seppä, Bjune, Telford, Birks, & Veski, 2009; Borzenkova et al., 2015). A study of a collated database of pollen records from Northern Europe, published by Heikki Seppä et al. (2009), obtained from core sampled lake sediments, indicated that in 5500–4500 BCE pollen levels were at a peak (Fig. 4). The pollen levels suggest that back then the average annual temperature may have been up to 2 degrees Celsius higher than in 1950 CE. The amount of pollen fluctuated periodically until after 2950 BCE the levels began to decrease slowly and steadily. According to the study, the average annual temperature dropped below the 1950 CE level after 750 CE. Pollen levels continued to decrease until a new upward trend began after 1700 CE. The chronology of the pollen record was based on AMS-dating of different stratigraphic levels within the core samples (Seppä et al., 2009; see also Chapter 9.3). However, pollen-based temperature reconstructions rely on proxy data, so they are not exact correlations with actual temperatures.

In a more recent study, different models for comparing pollen records and average temperatures suggest similar results, although the differences highlight the uncertain nature of reconstructing the past climate (Zhang, Renssen, Seppä, & Valdes, 2018). Another recent study utilizes data that shows summer temperatures gradually cooling from 5500 BCE onwards, while winter temperatures (Fig. 3) rose slightly, but with more short-term fluctuations (Kuosmanen et al., 2018: Fig. 2).

Data published by NASA (2020) shows that the annual temperature average has risen 2 degrees Celsius between 1950 and 2019 in Central Fennoscandia. Thus climate change of 5500 BCE – 1000 CE seems to have been roughly comparable to 1950–2019, with the opposite trend and a vastly different timescale. The differences observed in the last decades have occurred nearly a hundred times faster than during the time span of the current study. Thus the two periods may not actually be comparable. It remains to be seen, centuries from now, whether the current fluctuation is only short-term, but its correlation with the proliferation of industrial air pollution suggests the opposite.

[1] It is assumed here that in relevant studies the zero-point of Before Present chronology is 1950 CE as per the agreed upon standard involving radiocarbon dating. Still, the studies cited in this section do not explicitly state this.

Fig. 3. Mid-winter temperatures on the Bothnian Arc regularly plummet to -20 degrees Celsius even as the overall climate continues to warm (photo by author).

Averages during the study's timespan undoubtedly hide several warmer and colder periods (e.g. Seppä et al., 2009), which may have had consequences for the flora and fauna. According to a climate reconstruction of the Northern Hemisphere by Marcott, Shakun, Clark, & Mix, (2013), short-term cold periods may have occurred periodically, with cold peaks in 6250 BCE, 5450 BCE, 3950 BCE, 3550 BCE, 2850 BCE, 1650 BCE, 1150 BCE, 250 CE, 1150 CE, and 1450 CE (Marcott et al., 2013: Fig 2: I). Northern Europe's climate reconstruction model by Seppä et al. (2009) suggests that cold periods may have peaked in 6250 BCE, 4950 BCE, 3300 BCE, 1850 BCE, 600 BCE, 450 CE, 1000 CE, and 1750 CE (Seppä et al., 2009: Fig. 3–5). On the other hand, climate models arguably continue to lack sufficient resolution to verify short-term changes (see e.g. Zhang et al., 2018).

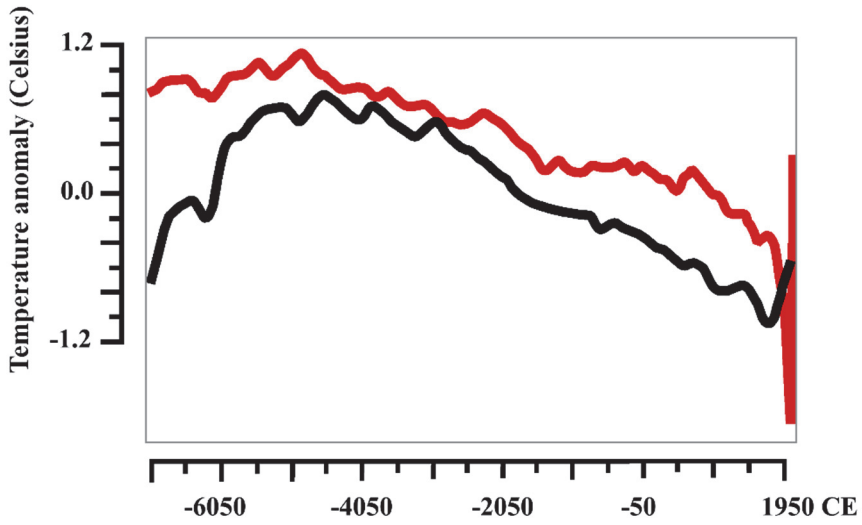

Fig. 4. Temperature anomaly curves for the Northern Hemisphere by Marcott et al. (2013) in red (upper) and for Northern Europe by Seppä et al. (2009) in black (lower).

Nevertheless, what may or may not hold up in future research is that very early in our timeframe, ca. 5500 BCE, the climate was about as warm or slightly warmer as during the early 21st century CE. Since, unlike with the current climate change, vegetation had had time to adapt, more broad-leaved tree species likely grew throughout the Bothnian Arc and in the lower elevations of Central Fennoscandia. Sometime before 4000 BCE temperatures began to gradually decrease with an overall cooling trend lasting until the trend became inverted during the industrial period, after 1850 CE. The short term, within a few centuries, variation of mean temperatures seems to have been within the ±1.5 degrees Celsius margin (e.g. Borzenkova et al., 2015). Mean temperature variations may hide several dramatic cold and warm phases, which may have had significant effects on fauna, including humans. Since research into the past climate is ongoing, this overview is likely to change significantly even within a few years (see e.g. Liu et al., 2014; Park et al., 2019).

4.2 Riverine agencies and life of the land

Turning to the other aspect of environment, it is probably not an overstatement to claim that in general neither we modern Finns nor Swedes in earnest think of nature as a living entity. Forests especially are generally regarded as a resource, a key source of wealth devoured for economic gain. Despite Finland and Sweden being two of the most forested countries in Europe, old-growth forests, with their flourishing biodiversity, are extremely rare due to logging and to a lesser degree the spread of agriculture. During the 20th century rivers throughout Finland and Sweden were dammed and harnessed for hydroelectricity, and vast expanses of marshland were dried to transform them to breeding grounds for the logging industry. Many animal species have been slow to recover from the brink of human-caused extinction or are headed there now. The 20th century saw the emergence of the particularly brutal form of animal exploitation, factory farming.

With this recent heritage that has taken place within only five generations, it is easy to turn a blind eye to the anthropogenic manipulation of nature and regard Fennoscandia as a mainly primeval environment, where humans, plants and animals live in relative balance and harmony. This is certainly how Finland is marketed for tourism purposes. This, however, requires exceptional self-deception. In the 90s, as a child, I was taught that animals do not have feelings, but instead they merely act on biological instinct. Dissenting views, with animals considered independent agents, were patronizingly dismissed. This hard-science view has turned out to be based on nothing but the lack of knowledge (e.g. Midgley, 1983). Instead, animals have been shown to exhibit a wide range of emotions and mental fluency, which undermines human exceptionalism (e.g. Safina, 2015; de Waal, 2019).

However, in this section, instead of focusing on past perceptions of organic beings, I wish to turn to things more alien to our modern eyes. Let us turn our focus on the life of geography, and, most compellingly, the life of rivers.

Historian Juhani Kortesalmi (2003) discussed riverine travel in Northern Finland prior to the damming of the major rivers. The region's rivers are often described as the main routes, conduits, between the coast and inland. Kortesalmi details a report from 1905 which asserts the distances of a day's journey on the rivers of Lapland as 30–70 km downstream and 20–50 km upstream. Travelling upstream was generally more laborious since much of the distance had to be traversed by punting the boat with a pole. According to Anders Johan Wathén, who traveled upstream on the Simojoki River in late July, the shallow rapids made travel

slow, but he still managed more than 100 km in four days. According to Wathén, the maximum cargo for such an endeavor was considered 140 kg (Kortesalmi, 2003: 51–53).

The rivers were tamed by winter when the waters froze and were covered by thick snow. These bright white lanes offered steady and level pathways to be travelled by ski and sled. In the dark winters of the Sub-Arctic, where daylight on the darkest days illuminates the landscape for only a few fleeting hours, the snow-covered lanes offered clearly marked routes in contrast with the surrounding black forests.

Rivers can be seen as maps in themselves. Kortesalmi (2003) described how the rivers that descend through the Bothnian Arc were conceptualized by those who traversed them. Each river consists of twists and turns, fast rapids and slow drifts, and each section was named accordingly. A traveler had to know only the sequences of names that the different parts of the river had in order to understand how to navigate them. According to Kortesalmi, 'a name was a message. The name told in advance how to proceed in an upcoming part of the river, what there was to be expected' (Kortesalmi, 2003: 55, my translation).

It seems probable that already in prehistory such oral maps of the Bothnian Arc and the whole coast of the Gulf of Bothnia existed. Each river outlet and estuary had their own name, a distance between them, and descriptions for different body parts of the rivers. Navigation in Central Fennoscandia may have been heavily dependent on the knowledge of the intricacies of the rivers. These rivers, fluid yet moody in the summertime or solid and tranquil during winter months, fan out from the Gulf of Bothnia in all directions, connecting the different outlets of the gulf coast to specific inland regions. To know the rivers is to know the geography, since islands, hills, marshlands, and lakes, and of course, settlements, can be located relative to the nearest river.

Among the easiest mnemonic devices are stories. Stories offer multiple contexts from which to deduce particularities which may otherwise be difficult to remember, such as simple individual place names. Although we lack the ethnographic record to really assert how the knowledge of the rivers was transmitted, the use of the word 'murder' in the naming of many rapids in the region suggests that the place was remembered as a murder-scene, where the culprit was the river itself (see also Kortesalmi, 2003: 56). It would only fit the general idea of oral tradition if such place names also carried with them intriguing and lively stories draped in mythology.

It is common for rivers to be assigned a temperament. Worldwide, amongst the most temperamental rivers are the two main conduits of China: the Yangtze and the Yellow River. The two vast rivers have enabled prosperity but have also caused calamities, depending on their mood. The recurring incomprehensively destructive flooding of the two rivers, worsened by anthropogenic factors, have swept away the lives of millions of people. Accordingly in the Chinese worldview rivers are seen as powerful deities or dragons capable of extreme destruction and also extreme generosity, when afforded with proper respect (see Ball, 2016). In Ancient Egypt, the whole state was harnessed to appease the Nile, who gave abundance but could always unpredictably change its mind (see Connah, 2015: 18–21, 70–75).

Fig. 5. Rivers channel not only water but also people and fauna. A summertime landscape with the Oulujoki River on the background (photo by author).

In contrast to these three vast and highly dualistic rivers, the rivers of Central Fennoscandia can be seen more akin to nurturing parent-figures. While some rivers do flood periodically, the flooding can only be considered catastrophic in the context of agriculture and permanent housing. It is likely that the prehistoric dwellers along the riverbanks understood the inconvenience of mild annual floods and set their dwellings at suitable elevations from the rivers, at a short yet respectful

distance. During prehistory, the temperament of the rivers, and their duality as protectors and punishers, was probably more prominent in the context of travel than in their relationship to riverside habitation.

The mild danger of flooding was vastly overcompensated by the abundance of fresh water and fish. Salmon runs on the rivers of the Bothnian Bay were legendary before most of the rivers were blocked by the hunger for hydroelectricity. It is doubtful whether the foragers of the north had to face starvation (see also e.g. Sahlins, 1972; Bird-David, 1990; Bird-David, 1992). Overfishing may have become a factor in declining salmon populations after the Iron Age ca. 1300 CE when commercialism began encouraging overproduction (see e.g. Hoffmann, 2005; see also Bergman & Ramqvist, 2017). Similarly the rivers provided fresh water until agricultural runoff turned the flowing waters brown.

It should be noted that in many instances, rivers are more multifaceted than just being providers of resources and lanes for movement. In 2015, the river Whanganui in New Zealand was given legal rights. This had been a long-time aspiration of the Whanganui people, the indigenous population who regard the river as their ancestor (see e.g. Iorns Magallanes, 2016). I argue that this view would have resonated with the prehistoric people of Central Fennoscandia, especially with the northern foragers, whose livelihood was not only heavily dependent on them, but more than that, their lives revolved around and within the rivers.

Ngā Roma Poa, a cultural mediator and river guide, in an interview for BBC Earth, explained her relation to the river Whanganui.

> As children we've always been brought up with the idea that every sort of living organism is just as important as you. Our *awa* [river] is very much a personality of its own, too. She's patient. She's consistent, she's persistent. You see the water sort of channel through rocks and it slowly cuts into these huge pieces. And then at times she just roars. She's very fierce in a way, but she does it so elegantly. It's how I would explain her. […] My relationship with the river has kind of been my life. I've always considered the river being my greatest teacher, dearest friend. (Ngā Roma Poa, in Evans, 2020)

It is impossible to know just what the communities of the Bothnian Arc thought about the rivers. The relationship depends much on the dwelling practices of communities. Much has been written about mobility and sedentariness of local groups, with the main conclusion being that habitation was more sedentary during the Neolithic than during previous and following periods of prehistory (see e.g. Norberg, 2008; Käck, 2009; Vaneeckhout, 2009; Holmblad, 2010; Mökkönen,

2011). On the other hand, there really is no proof that habitation was particularly mobile at any period during our timeframe of 5500 BCE to 1000 CE (see Käck, 2009). Interpretations of residential mobility are heavily dependent on ethnographic analogies, sometimes from practically unrelated contexts (Käck, 2009; see also Räihälä, 1997). I suspect that early on in Finnish archaeology, the model of prehistoric residential mobility was a projection from ethnographic examples of the 1700–1900 Sámi way of living, which most famously revolved around the seasonal migration of reindeer (consider e.g. Wallerström, 2020). Projecting such a model onto previous times, prior to the domestication of reindeer, would be problematic to say the least, if not outright invalid, especially in a coastal context.

Because the evidence for residential mobility in the region's prehistory is scarce, I argue that the local prehistoric communities should be studied as practically sedentary for two reasons. First, there was no overriding reason for seasonal migration since the rivers and the ubiquitous forests may have guaranteed a steady livelihood even during winter. Second, there are yet no archaeological means to determine whether two dwelling sites are actually products of the same habitually relocating community. Besides, a community can adopt a different identity at a new location (see e.g. Wengrow & Graeber, 2015). On the coast, where the number of ideal dwelling places, especially on river estuaries, is limited, seasonal mobility would carry with it the uncertainty of whether the dwelling places would remain unoccupied during the hiatuses.

I would argue that the stories of a river may have acted as the foundation for local identity. The communities alongside the same river may have shared an affinity. Similarly, communities of neighboring rivers could be seen partly through the personality of the corresponding river. They may have even heeded the river's example, whether calm and consistent or aggressive and unpredictable. On the other hand, it could also be argued that the communities may have sought to act in ways that would balance the personality of the river. In Paper IV, the placement of burial sites in riverine and promontory contexts is interpreted as circumstantial evidence for the existence of such acting environmental entities. Also, in Paper V I argue for a stronger emphasis on studying local material identities with delimiters based on geographical phenomena, such as rivers, which may be more alive than we assume.

4.3 Land uplift and shoreline displacement chronology of the Bothnian Arc (Paper II)

The rivers are part of a geography that was carved by glaciers and ice sheets that completely covered the region during much of the last 100,000 years of Upper Pleistocene. The core of the Weichselian glaciation, which at its maximum extent covered the whole Northern Europe, was on the western coast of the Kvarken. The ice was several kilometers thick, compressing the earth beneath it with unmatched force. Once the climate began to warm, the ice began to melt and recede. Fluvial action under the melting ice left in its retreat the overall topography of today.

Fig. 6. Land uplift may have affected local cosmology in prehistory when the effect was even stronger than today and more discernible within a lifetime (photo by author).

Early in the Holocene epoch, once the ice had gone, with only the glaciers of the Scandinavian mountains maintaining in permafrost, Central Fennoscandia was almost exclusively seabed. But the land that was compressed by the ice, and subsequently released, strived for equilibrium and began to rebound (see Fig. 6). According to research by geologist Mattias Lindén (2006), the highest Holocene shoreline in Norrbotten, is currently located over 200 m a.s.l. (meters above sea-

level). The land-uplift rate was strongest during the first few millennia, after which the effect decelerated due to the global sea-level rising gradually by 25 m. As the global sea-level settled sometime after 5000 BCE, the apparent land uplift became slightly stronger, but levelled into gradual deceleration after 4000 BCE (see Lindén, 2006; although cf. Chapter 9.3).

The land uplift effect has remained constantly stronger than the periodical rising of the sea-level. Initially, the postglacial rebound was faster, but in time its velocity has decreased. Nonetheless, some parts of the Kvarken continue to experience apparent land uplift, in relation to the mean sea-level, of up to a meter per century. The strong land uplift of the region has left the coastline of the Gulf of Bothnia in a constant state of flux, not necessarily noticeable from a human perspective, but certainly so from the archaeological point of view.

Paper II focuses on this effect and its relevance in local archaeology. The study consists of a re-evaluation of the shoreline displacement chronology of the eastern coast of the Gulf of Bothnia. The chronology is based on an equation by geologist Marjatta Okko, published in 1967. In both his licentiate thesis (Okkonen, 1998a) and doctoral dissertation (Okkonen, 2003), archaeologist Jari Okkonen adopted the equation, applying apparent land uplift rates determined from mareograph, or tidal gauge, records of the 20th century, which had been published by Seppo Kääriäinen (1982) and Vermeer et al. (1988). Okkonen's brilliant application of shoreline displacement chronology produced the first long-term narrative of the region's archaeology and acted as a major inspiration for my own research. The chronology itself has been applied in many archaeological studies focusing on the coast of the eastern Bothnian Bay (see e.g. Ikäheimo, 2002; 2005; 2015; Ojanlatva & Alakärppä, 2002; Äikäs & Ikäheimo, 2005; Kuusela, 2009; 2013; Rantanen, 2014).

The reason for conducting the study presented in Paper II arose from the need to strengthen the underlying methodology of the shoreline displacement chronology and to improve its accuracy. To me, the problem in Okkonen's chronology was the limited number of benchmarks. Benchmarks are important in calibrating the shoreline displacement curve, and they need to be established with data that clearly indicates the shoreline placement in a given time. Okkonen used the basin isolation date of Lake Kalliojärvi in Kauhava (see Glückert, Rantala, & Ristaniemi, 1993) as the only independent benchmark, probably because at the time such data was simply difficult to come by.

When I began the dissertation and was ready to apply the chronology, it seemed to me that the growing number of radiocarbon dates from the region might have made Okkonen's chronology outdated. The radiocarbon date record that was

accessible to Okkonen was extremely limited. With this in mind I collected the relevant data of 65 radiocarbon dates from 28 archaeological sites from the eastern Bothnian Arc and studied their connection with the shoreline. It became clear that the contexts of only a small number of dates could be definitely argued to signify the actual shoreline. The site of Purkajasuo [972010012] and its neighboring Purkajasuo, Korvala [972010038] in Oulu provided a sufficient archaeological benchmark with four radiocarbon dates. The two sites contained land-based dwellings adjacent to Neolithic fishing weirs or traps that had been preserved by the surviving wetland (see e.g. Koivisto, 2017).

Four dates as a benchmark is clearly not enough. Three further benchmarks were adopted from basin isolation dates. But, what transpired was a mismatch in the two records, the archaeological and the geological (see Chapter 9.3). Since the radiocarbon dated samples of the archaeological record were taken from much more varied contexts than the geological isolation dates, it seems more likely that the error lies in the latter. The observed errors forced my hand to mostly abandon the geological benchmarks, rooting the chronology in only the four dates from Purkajasuo. However, by plotting the rest of the archaeological radiocarbon date assemblage onto the evaluated curves and seeing whether the probability distributions of the dates placed archaeological samples under the mean sea-level with high probability, the validity of the chronology was established.

To my surprise Okkonen's chronology by and large withstood the trials. I perceived a slight mistake by Okkonen concerning the land-uplift rate of the Oulu region, but since Okkonen extended the chronology only to 2000 BCE, this had not had a meaningful effect on his study. What I found even more surprising was that the chronology performed well at least up to 4000 BCE. Prior to this, however, it is likely that the curve's upward projection should gradually shift to the opposite trend, when, going backwards in time, sea-level rise was temporarily accelerated by North American deglaciation (e.g. Salonen, Eronen, & Saarnisto, 2006: 29–30).

In my judgment the most valid curve evaluated in Paper II is based on the high range rates of Kääriäinen (1982), which I used in the later studies (Papers III, IV, and V; I used Vermeer et al.'s [1988] median values with high and low values to determine the error margins in Paper I). Furthermore, the mareograph-based apparent land-uplift rates seemed to be anomalous on the eastern coast of the Kvarken. The curves in that particular region are too steep and placed the four radiocarbon date samples from the site of Kangas [236010002] in Kaustinen beneath the contemporary sea-level with high likelihood. The best rate for keeping Kangas dry was the corrected high value of Kääriäinen (1982) for the Oulu region.

A study of land uplift in the Kvarken archipelago by Markku Poutanen and Holger Steffen (2016) also suggested similar uplift rates from Oulu to Vaasa, despite Vaasa being closer to the epicenter of the isostatic rebound (see Poutanen & Steffen, 2016: Fig. 5b). It has to be noted that this trend was only observable in the *absolute* GPS-measured land uplift rate (based on Lidberg, Johansson, Scherneck, & Milne, 2010) and not in the *apparent,* relative to mean sea-level, uplift rate.

It seems logical to me that if we are to calculate the land uplift of the past 7,500 years based on recent uplift measurements, we should synchronize relative regional rates from the absolute land uplift and not from the apparent uplift, which is arguably more affected by temporary changes in wind patterns. I came to this conclusion only after I drew up Appendix 3 of Paper II, which includes the uplift curves for the regions of Raahe and Siikajoki, which I now consider as probably erroneous. In the following studies I have tended to use the curve of the Oulu region as far south as Kokkola as suggested by the absolute rates in Poutanen and Steffen (2016: Fig. 5b). For Kemi-Tornio region I used the rates presented in Paper II and for the Simo region I used the average of the previous two.

For application on the Swedish coast I calculated the relative difference in the GPS measurements between the uplift rate of the specified region and the rate in the Oulu region, and applied the difference to the established Oulu curve. For example, the absolute GPS-measured uplift rate for Oulu is indicated as 9.7 mm/year, while in Umeå it is 10.9 mm/year (see Poutanen & Steffen, 2016: Fig. 5b), so in Umeå the uplift is 12.4% faster. In the Kemi-Tornio region the rate is 3.6% faster than in Oulu, only slightly contradicting the rates in Paper II, in which the difference in the uplift rates was 4.2%.

Lastly, in this synthesis, to verify the uplift presented in Fig. 7 and Table 2, I used a smaller set of radiocarbon dates from northern coastal Sweden to make sure that none of the samples ended up underwater. This was only a preliminary inspection and is not comparable to the analysis in Paper II. The additional radiocarbon dates are presented in Table 3. The radiocarbon dates were calibrated with CALIB Radiocarbon Calibration Program v. 7.1 (Stuiver, Reimer, & Reimer, 2020). As none of the dates in Table 3 ended up below sea-level, the veracity of the elevation model presented in Fig. 7 is at least tentatively established.

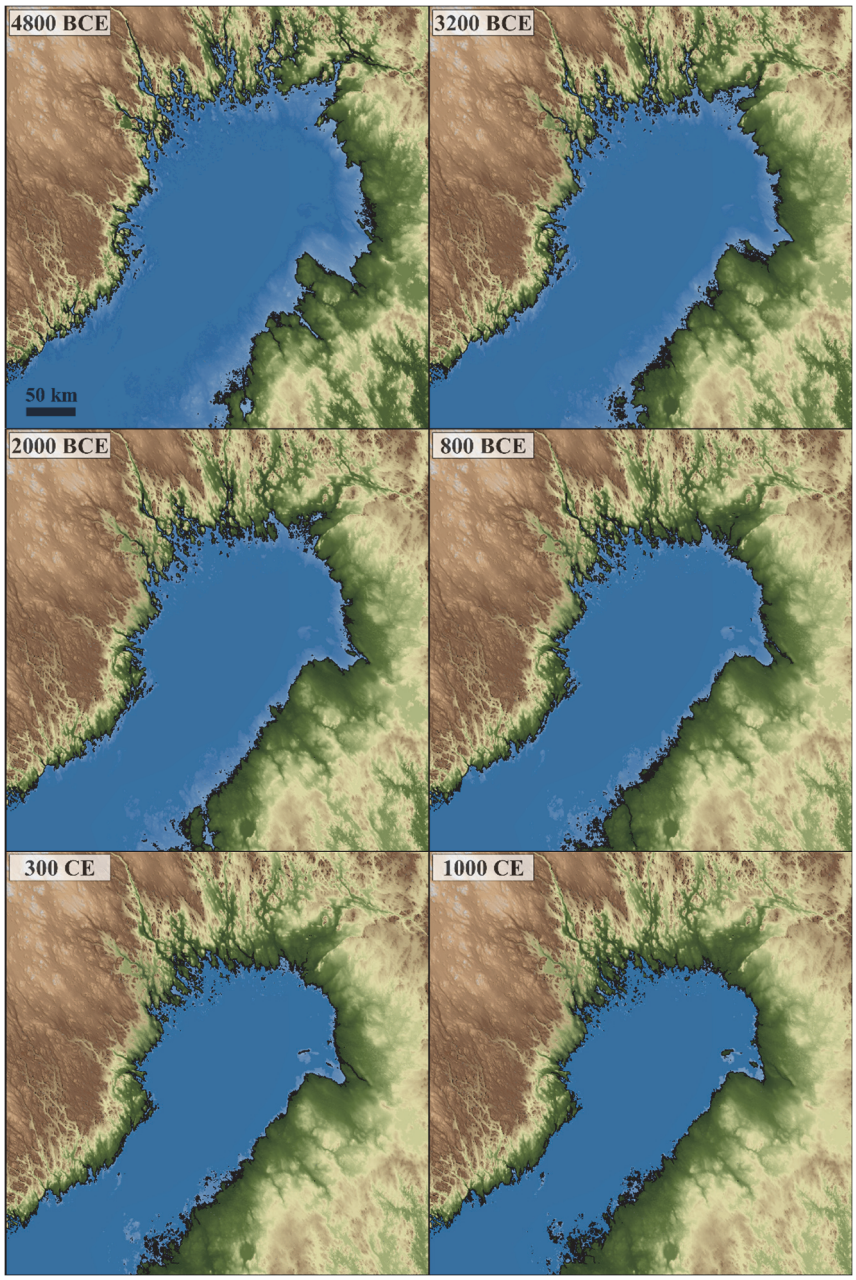

Fig. 7. Terrain elevation model (NLSF and Landmäteriet) of the Bothnian Arc from different time periods (last page).

Table 2. The surface area of new land emerging in the archipelago and the mainland of the Bothnian Arc and the Kvarken, as presented in Fig. 7.

Timeframe	New surface area (km^2)	Yearly average (km^2)
4800–3200 BCE	11 814.8	7.4
3200–2000 BCE	8 482.4	7.1
2000–800 BCE	7 099.6	5.9
800 BCE – 300 CE	5 828.2	5.3
300–1000 CE	3 402.3	4.9

Archaeologist Petro Pesonen also produced his own land uplift curves for the eastern coast of the Bothnian Arc (Pesonen, 2016). His curves were based on assemblages of regional archaeological radiocarbon dates, with added support from several basin isolation dates. The problem is that not only is there a possible systematic error in the basin isolation dates (see Chapter 9.3), but Pesonen seems to have made a category error in confusing both as the same marker, i.e. the location of the contemporary shoreline. In reality there is no guarantee that archaeological sites by default actually resided strictly on the shore. The most anomalous dates, in this context meaning those that the curves showed could not have possibly resided on the shore, were omitted by Pesonen.

That the curves were plotted *through* the major probability ranges of the dates, leaving many archaeological samples underwater with high probability, reveals that the curves were probably drawn rather hastily. The clearly erroneous method was duplicated in a study by Tallavaara and Pesonen (2018: Fig. 3), with the computational uplift curve placing roughly half of the archaeological dates underwater. This is simply due to the underlying logic of the BACON algorithm (see Billor, Hadi, & Velleman, 2000) being incompatible in this context.

Nevertheless, Pesonen's shoreline displacement curves may represent short-term fluctuations of the sea-level better than mine. The equation by Marjatta Okko (1967) is linear in the sense that it does not take eustatic sea-level change into account. But these short-term events should in the future be extrapolated from the environmental record, not strictly from the archaeological record, unless many more actual shoreline indicators, such as Purkajasuo, can be used as benchmarks.

Table 3. A small assemblage of radiocarbon dates from the western coast of the Gulf of Bothnia that I used for a preliminary verification of the Swedish shoreline chronology.

Site	m a.s.l.	BP +/-	Sample ID	Reference
Karl Gustav 264	46	3890±70	St-13391	Norberg, 2008: 92
Nedertorneå 315	52.4	3910±70	Ua-14513	Norberg, 2008: 99
Grundsunda 125	14	1575±65	Ua-2721	Johansson & Grundberg, 2011
Själevad 22	21.3	1515±31	Ua-41044	Ramqvist, 2016
Umeå stad 323	35+	3010±40	Ua-33420	Ramqvist, 2017
Hortlax 73	33.7	2895±34	Ua-53994	Palmbo, pers. comm. 2020
Umeå stad 77	37.8	3095±35	Ua-36136	Ramqvist, 2009
Umeå stad 600	39	3385±75	Ua-15399	Andersson, 2000
Överkalix 451	61.5	4955±100	Ua-2635	Nordqvist et al., 2011
Överkalix 393	84.7	6150±60	Ua-10795	Liedgren, 2014
Överkalix 977	90.2	6230±65	Ua-10794	Liedgren, 2014
Älvsby 805	94	6150±100	Ua-2502	Liedgren, 2014

5 Overview of Central Fennoscandian prehistory

Table 4. Periodization of the Central Fennoscandian past used in this study. The main timeframe extends from the Late Mesolithic to the "Mid-Iron Age forager gap."

Periodization	Sub-periodization	BCE/CE
Pleistocene	------	Until ~9000 BCE
Mesolithic Age		9000–4000 BCE
	Early Mesolithic	9000–6500 BCE
	Middle Mesolithic	6500–5200 BCE
	Late Mesolithic	5200–4000 BCE
Neolithic Age		4000–1700 BCE
	Early Neolithic	4000–3200 BCE
	Middle Neolithic	3200–2300 BCE
	Late Neolithic	2300–1700 BCE
Bronze Age		1700–300 BCE
	Early Bronze Age	1700–1200 BCE
	Middle Bronze Age	1200–700 BCE
	Late Bronze Age	700–300 BCE
Iron Age		300 BCE – 1300 CE
	Early Iron Age	300 BCE – 200 CE
	Middle Iron Age	200–800 CE
	(Gap in forager record)	**(~400/500–800 CE)**
	Late Iron Age	800–1300 CE
Historical Age		1300–1950 CE
	Church Period	1300–1600 CE
	Town Period	1600–1850 CE
	Industrial Period	1850–1950 CE
Digital Age		1950 CE –
	Cold War Period	1950–1990 CE
	Global Period	1990–2020 CE

In this chapter my aim is to present the regional narrative of prehistory based on up-to-date archaeological research. I will apply the theoretical framework of Chapter 3 only in situations where my own contributions warrant it. The narrative derives mostly from prior interpretations.

The first problem facing the archaeologist studying the region is that there are no clear overviews of the region's prehistory. The modern border between Finland and Sweden has created a break, most notably in the accepted starting point of the Neolithic Age. This dissertation aims to correct some contradictions between the two national archaeologies. In the same light, the periodization in Table 3 is based solely on material culture to limit the contrast between the historical written and the archaeological prehistoric records.

The Mesolithic in Sweden lasts until 4000–3900 BCE when agricultural practices emerged in Southern Fennoscandia (Welinder, 2009: 43; Larsson, 2014: 3, 23–37; Olofsson, 2015: 3). This is based on the Western European periodization, which was established following intense debate in the early half of the 20th century (see e.g. Peake, 1934). In contrast, Finnish archaeology currently places the end of the Mesolithic earlier at 5300–5200 BCE, when pottery use began in Finland (Torvinen, 2000; Carpelan, 2002: 26; Pesonen & Leskinen, 2009; Halinen, 2015: 21). The Finnish definition for the pottery-based Mesolithic-Neolithic transition, for unaccounted historical reasons, derives from the Russian definition (see e.g. Piezonka et al., 2017). According to archaeologists Olga Lozovskaya and Vladimir Lozovski (2016), 'the beginning of the Neolithic in the Russian forest zone is traditionally associated with the appearance of pottery production, because the productive economy appeared there only at the end of the Bronze Age' (see also Dolukhanov, 2008: 280–281). Considering the nature of the society of the Soviet Union at the time (see e.g. Figes, 2008; Nordqvist, 2018: 29–41), it would not be surprising if the pottery-based definition was adopted partly for political purposes to compete with the Western European archaeology.

With the shaky origins of the Finnish terminology, I believe it is more pertinent to anchor the chronology within the regional archaeological record. Here the Mesolithic-Neolithic transition is synched with the Swedish chronology, since the cosmological changes that took place in Central Fennoscandia in 4000–3800 BCE, mainly the formation of large villages and, in context with the spread of agriculture to Southern Sweden, the extension of material networks to agricultural regions, is assessed to outweigh the effects of the emergence of pottery (cf. Jordan & Zvelebil, 2009: 45–68; also Jordan & Gibbs, 2019). Since the issue is solely about terminology invented by archaeologists to help communication within the field, I am not dismissing recent research into the so-called Neolithization process in Northern Fennoscandia (see Herva, Nordqvist, Lahelma, & Ikäheimo, 2014; Herva, Mökkönen, & Nordqvist, 2017). Neolithization in this context is an amalgam of activities which were practiced most notably during the Neolithic.

5.1 Cultural divergence of north and south from the 6th millennium BCE to the 2nd

By the beginning of the timeframe of this study, ca. 5500 BCE, humans had inhabited the region for at least two millennia (Bergman, Olofsson, Hörnberg, Zackrisson, & Hellberg, 2004). The archeological record of the Early and Middle Mesolithic in Central Fennoscandia consists almost exclusively of activity sites with stone tools and discards, and fireplaces along with food waste. While I have not collated data from the previous Mesolithic, altogether 35 Late Mesolithic sites are known at the moment from the Bothnian Arc, with burial sites comprising a significant proportion of the record (Fig 8; see also Lindgren, 2010: 23).

The scarcity of evidence of contemporary structures suggests that Mesolithic dwellings were mostly light and temporary, perhaps erected by laying hide covers on a conical framework of wooden stakes. An uncertain yet possible such dwelling structure was excavated in 1947 along the Oulujoki River in eastern Bothnian Arc at Pyhänniska [889010078] (Fig. 8: #1). The dwelling was demarked by up to ten post holes surrounding a round hearth, seemingly forming a 6-m diameter *goahti*-style hut (see Meinander, 1948). Pottery found in the same context was later dated to 5300–4800 BCE (6140±105 BP, see Torvinen, 2000: Appendix 2). Surviving evidence for such structures is extremely difficult to perceive, so their rare occurrence in the archaeological record should not indicate contemporary rareness.

Whether Mesolithic communities were mobile or sedentary or anything in between is a difficult question. Interpretations of levels of mobility are mostly projected from ethnographic examples. Foragers are in general presented as highly mobile in ethnographic studies (see e.g. Kelly, 2013: 77–108). The problem with such projections is that the ethnographies they project may be strongly biased.

Ethnographic studies began to be conducted some centuries after epidemic diseases spread by Europeans, as well as mass killing, had decimated indigenous populations, while colonial exploitation had in many places driven local fauna to the verge of extinction. To determine levels of mobility in Mesolithic Fennoscandia by analogies with such an ethnographic record is to assume that the population had constantly experienced a catastrophe, a traumatic bottleneck, perpetually decimating the population to 1/10th of what it was a few centuries ago without ever reaching normalcy. Such a bias—figuratively speaking a *statistical population purgatory*—may have radically affected how we conceptualize prehistoric communities, including our understanding of how densely populated prehistoric worlds tended to be. The trend in Fennoscandian archaeology is that coastal groups

are frequently understood as more sedentary than inland groups, and Mesolithic groups as more mobile than Neolithic ones (Damm, 2006a; Damm, 2006b; Vaneeckhout, 2008; Hertell & Tallavaara, 2011). The question of whether these interpretations are based on solid foundations requires a study of its own.

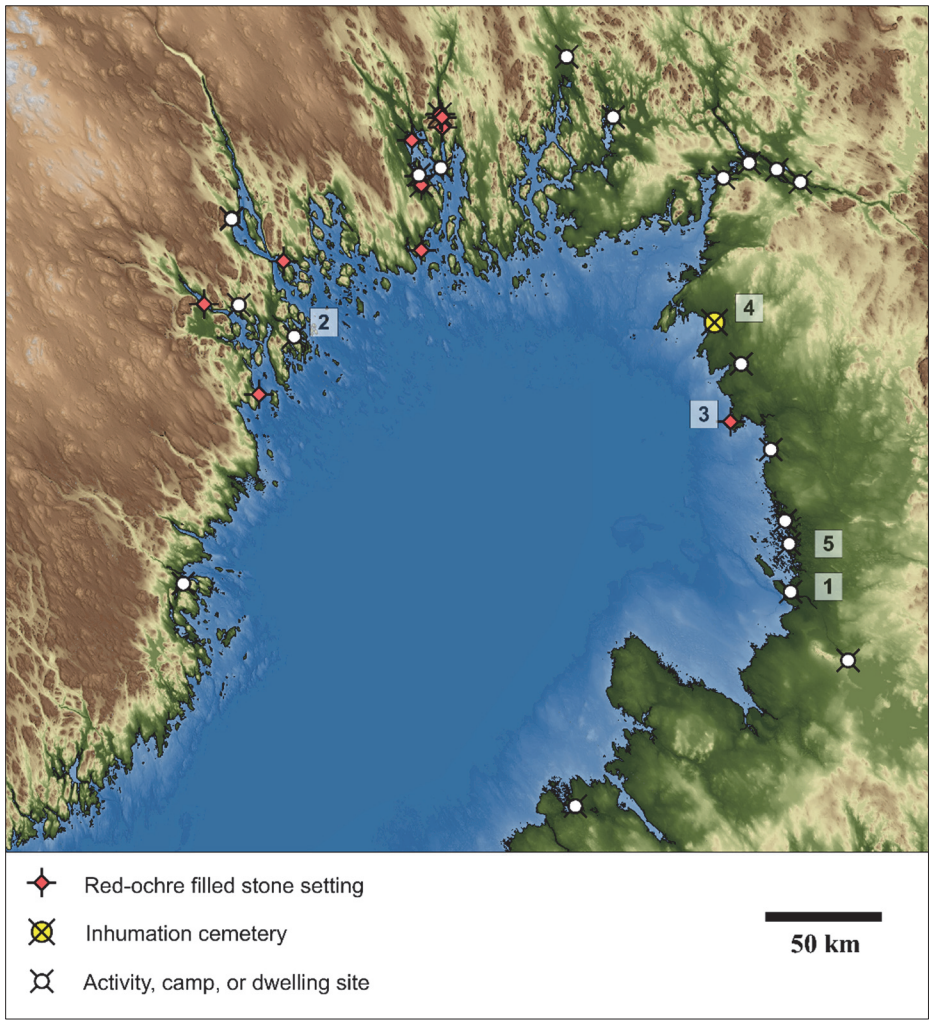

Fig. 8. A map of the Late Mesolithic Bothnian Arc. The numbered sites are mentioned in the text: 1. Pyhänniska; 2. Överluleå 184:1; 3. Keelaharju; 4. Tainiaro; 5. Vepsänkangas. DEM by NLSF and Landmäteriet.

Dwelling depressions (A.K.A. house pits), signifying the remnants of semi-subterranean dwellings (A.K.A. pit-houses), contribute greatly to the later Neolithic record of northeastern Fennoscandia. The remnants represent structures, whose foundations were dug slightly into the ground with the loose soil piled into surrounding embankments. It is possible that already in 5500 BCE individual semi-subterranean dwellings were used. This is at least according to radiocarbon dates from two apparent dwellings on the northwestern coast of the Bothnian Arc at Överluleå 184:1 (Fig. 8: #2; see Norberg, 2008: 77–79). It could also be that the two are actually later inland riverine dwellings, since only a few radiocarbon dates were collected from the partially mixed context. No similar contemporary sites have yet been found on the eastern side of the gulf. On the other hand, in Arctic Norway, the earliest verified examples of the semi-subterranean dwelling tradition date to ca. 7000 BCE (Skandfer, 2012: 158; see also Fretheim, 2017). A few unverified traces of semi-subterranean dwellings in Central and Southeastern Finland have been dated to the beginning of the Mesolithic in the 9th millennium BCE (see Kriiska, Rostedt & Jussila, 2016; Rostedt & Kriiska, 2019).

The local communities buried at least some of their dead in shallow pits decorated with red ochre, a tradition shared throughout the coast of the Baltic Sea (Zagorska, 2008; Ahola, 2019). In addition several local traditions can be discerned during the Late Mesolithic. Especially on the northwest coast of the Bothnian Arc some of the dead were buried in red ochre filled stone settings, which date to 5300–4300 BCE (Fig. 8; Liedgren, 2014). Also a similar stone setting with red ochre has been found on the eastern coast at Keelaharju [139010014] (Fig. 8: #3; Mökkönen, 2013). There may be others, but more excavations of stone settings in the Late Mesolithic coastal zone are needed.

These stone setting burials apparently represent individual burials. In contrast, on the northeast coast of the Arc, the site of Tainiaro [751010040] (Fig. 8: #4) has revealed at least 40 probable inhumation burials. The unexcavated parts of the site may contain more than a hundred additional burials (see Paper IV). As of yet Tainiaro stands as the only such cemetery site in the region's prehistory.

Quartz was the most commonly used and available stone material, and it remained so until 300 CE. During the period of 5500–4000 BCE, also other local rocks were used and experimented with. The greenstone of Tervola—referred to here and in the related papers as mafic tuffite—was adopted locally as a general tool production material, but its use was later reserved almost solely for the fashioning of elongated Bothnic tools (see Paper V). The use of flint, a nonindigenous stone in the region, seems to have been sporadic, but individual

finds suggest that the related long-distance material networks existed either throughout Mesolithic or at recurring intervals. Ceramic use began around 5200 BCE on the eastern coast of the gulf (e.g. Torvinen, 2000). Pottery use in Southern Scandinavia may have begun slightly later around 5000–4500 BCE, but in both regions its early use seems to have been limited (Stilborg & Holm, 2009).

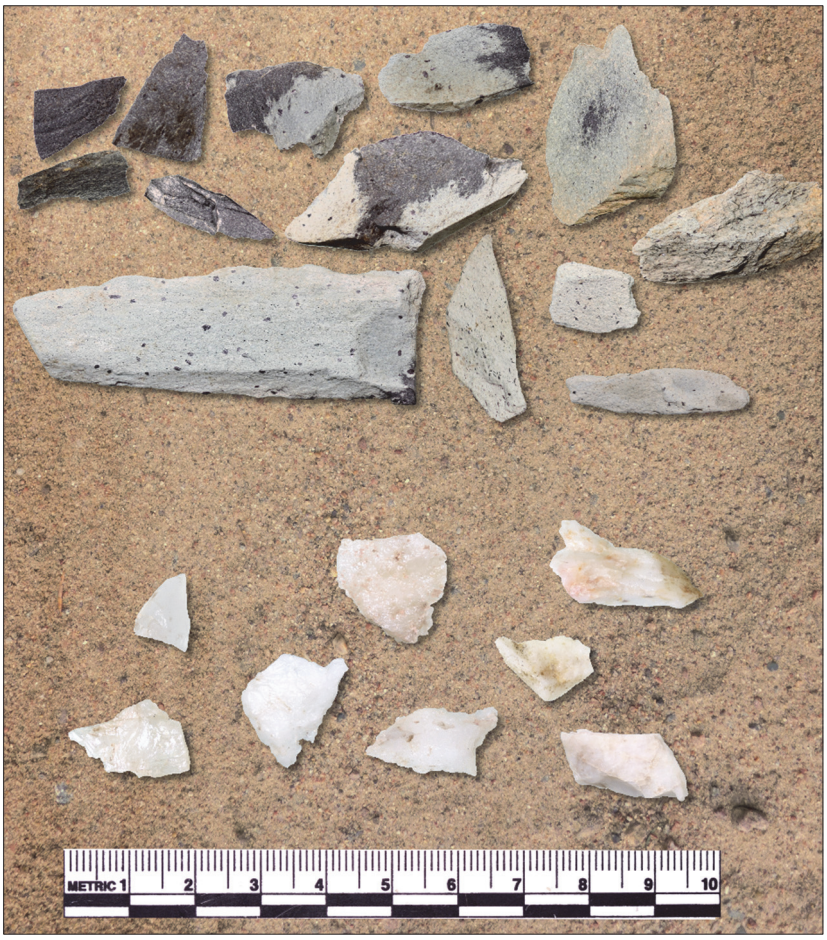

Fig. 9. A selection of Late Mesolithic discards of mafic tuffite (above) and quartz (below) from Tainiaro (figure from Paper V).

Animal bone finds indicate that hunting, fishing, and sealing were practiced in varying forms during the Late Mesolithic. For example, in the archipelago north of

the contemporary Oulujoki River estuary, at Vepsänkangas [973010046] (Fig. 8: #5), locals seem to have specialized in bird catching (Ukkonen & Mannermaa, 2017: 82). Dogs were the only apparent domestic animals, but they may have been of vital importance not only as members of the community but also as hunters (e.g. Mannermaa, Ukkonen, & Viranta, 2014). Evidence of wild plant use has not survived from this era with the exception of hazelnut remains (Vanhanen, 2019: 51–53), but plants and herbs were probably more widely used as food, spice, and medicine than is currently indicated by the archaeological record.

It seems likely that certain plants were seen as more useful than others. For example nettle and hemp could have been cultivated by clearing more space for them to grow, but it seems doubtful that staple food crops were grown (see e.g. Alenius, Mökkönen, Holmqvist, & Ojala, 2017). The net from Antrea in the Karelian Isthmus (see e.g. Miettinen et al., 2008), dated to the Early Mesolithic, indicates that plant-based crafting had already reached a higher level than suggested by the relative simplicity of contemporary stone tools. Wooden or woven baskets or skins may have been used as vessels long before ceramics (see e.g. Pritchard, 1974: 109–120).

The first rather abrupt break in the region's prehistory occurred in 4000–3900 BCE. Agriculture and pastoralism spread from mainland Europe into Southwest Fennoscandia (e.g. Bonsall, Macklin, Anderson, & Payton., 2002; Hallgren, 2012: 139), creating a boundary between the overall subsistence forms, which during the millennia inched its way further north. During the 4th millennium BCE the frontier of the agricultural zone ran roughly from Oslo to Stockholm. The groups south of this line experienced social and material transformations which were unrivalled elsewhere in Fennoscandia. The longhouse building tradition, famous in its Viking Age manifestation, began already at this stage (Larsson, 2014: 39–47).

In Southern Scandinavia, massive burial monuments, such as long-barrows, dolmens, and passage graves (see Blank, Sjögren & Storå, 2020; also Fig. 11: Neolithic stone burials), were constructed, possibly indicating the formation of territories and the emergence of small-scale polities (Olsen et al., 2012: 161–170; Larsson, 2014: 54–69). Finely crafted copper axes were distributed from Central Europe (Welinder, 2009: 156), indicating the introduction of metallurgical influence, while also communicating symbolic—and possibly physical—force and violence towards people and environment. Cremation became a practiced form of burial in the southern agricultural zone (e.g. Larsson, 2014: 57). All in all, a varied change in cosmology took place in the south (Paper III; also Persson, 1999: 149–152; Welinder, 2009: 162, 175, 180–181).

Simultaneously with the Early Neolithic changes in Southern Sweden, new forms of existence were incorporated throughout the rest of Fennoscandia. Villages of semi-subterranean dwellings were established mainly on the Eastern and Northern Bothnia Arc, as well as further north on the Arctic coast (Damm et al., 2019) and in smaller numbers on the shores of Finnish Lakeland (see e.g. Pesonen, 2002; Norberg, 2008; Käck, 2009; Vaneeckhout, 2009; Mökkönen, 2011; Paper V). Artistry was expressed in rock art and decorations on ceramic vessels (see e.g. Lahelma, 2008; Pesonen & Leskinen, 2009), although both forms of activity share Paleolithic roots. The earliest rock art sites in Fennoscandia may date from the very early Mesolithic (see Gjerde, 2010: 183–197; although cf. Goldhahn, 2017).

In the Early Neolithic, interregional material networks intensified in the north, with flint (Kinnunen, Tynni, Hokkanen, & Taavitsainen, 1985: 48–52; Costopoulos, 2003: 50; Mökkönen & Nordqvist, 2016), Onega green slate (Tarasov, 2015), small copper items (Nordqvist, Herva, Ikäheimo, & Lahelma, 2012; Nordqvist & Herva, 2013), and Baltic amber (Núñez & Franzén, 2011) spreading widely in the region. On the Bothnian Arc, Bothnic tools became a mainstay of the local stone tool repertoire, and such tools found their way even to the Arctic coast and to the Gulf of Finland. Red slate, which is unique to the regions west of the Gulf of Bothnia, was skillfully fabricated into knife blades and distributed throughout Central Fennoscandia (Hallgren, 2008: 255–260; Underdal, 2018). Shared similarities in the material cultures of the forager region indicates a level of connectedness of the overall northern cosmology (Herva et al., 2014; also Herva & Lahelma, 2020).

Although these northern communities seem to have maintained foraging subsistence, experiments with plant cultivation may have intensified in the south-eastern Finnish Lakeland (Mökkönen, 2010; Alenius, Mökkönen, & Lahelma, 2013). If such experiments were undertaken, they did not lead to Southern Scandinavian style agriculture (Lahtinen & Rowley-Conwy, 2013; Lahtinen, Oinonen, Tallavaara, Walker, & Rowley-Conwy, 2016), and foraging continued to provide the main subsistence for communities throughout the region (Herva et al., 2014: 144). Yet, agricultural products may have spread through material networks (e.g. Stilborg & Holm, 2009). The extent of the contemporary material networks is indicated especially by Baltic amber, which was increasingly common in Central Fennoscandia during the Early Neolithic (see Núñez & Franzén, 2011). Also, in the Finnish Lakeland region, imported flint makes up approximately 2/3 of the excavated debitage during 4000–3500 BCE, with only a few flint fragments found from other Stone Age eras (Mökkönen & Nordqvist, 2016).

Fig. 10. A totem guards a re-creation of a Neolithic rowhouse at the Kierikki Stone Age museum near Oulu (photo by author).

As I previously indicated, the eastern and northern shores of the Gulf of Bothnia saw the emergence of coastal villages, seen nowadays as sites with dozens or even hundreds of closely lying remnants of semi-subterranean dwellings (e.g. Liedgren, 1995: 118; Pesonen, 1999; 2002). These are also found in Arctic Norway and the Finnish Lakeland, where the sites tend to be smaller than along the Gulf of Bothnia (see Olsen, 1994; Simonsen, 1996; Norberg, 2008: 162; Mökkönen, 2011). Nevertheless, according to Damm et al. (2019), in Finnmark county alone, at least 2051 semi-subterranean dwellings have been found.

The coetaneous size of the Neolithic villages is unclear, with most suggestions ranging from 5 to 10 households per village at one time (Olsen, 1994: 86; Pesonen, 2002: 26; Halinen, 2015: 73; see also Mökkönen, 2011: 37 including references). Recently found ring-shaped villages in the Oulu region suggest coetaneous sizes of up to 20 dwellings (see Pesonen, 2017), while in extreme examples the numbers may have been more than a hundred (see Pesonen, 1999).

The gulf region is unique in the scale of its village aggregation (Núñez & Okkonen, 1999; Pesonen, 2002: 25–27; Norberg, 2008; Vaneeckhout, 2009). There

are over 110 sites, and counting, with ten or more semi-subterranean dwellings in the region. A total of over 3,000 dwellings are situated in hospitable locations along the 500 km fishhook-shaped coastline. The Gulf of Bothnia and Arctic Norway were undoubtedly the core regions of this particular housing tradition.

Fig. 11. Distribution map of the main material indicators of the Fennoscandian Neolithic, and the shaded extent of the Corded Ware Culture. Site data based on national registries, and DEM from DIVA-GIS.

The western coast of the Bothnian Gulf and the adjacent mountain regions seem to have contained communities of what may be called a liminal culture, adhering to their own technologically conservative Mesolithic forager logic. Some of these

western inland regions seem to have even resisted the use of pottery until the 3rd millennium BCE (Stilborg & Holm, 2009: 328–330). Local variants of semi-subterranean dwellings were used sporadically, with burnt rocks piled into dwelling embankments (so called *skärvstensvall*, e.g. Sjöstrand, 2011; Underdal, 2018).

These individual dwellings, and the lack of pottery in the region, may represent more mobile forager communities of the mountains and highlands. Several trapping pit systems radiocarbon dated to the Early Neolithic reside in the highlands (Hallgren, 2008: 255–260). On the other hand, red slate blade production seems to have been concentrated on the western coast of the Bothnian Sea, especially around the river valley of Ångermanälven (Hallgren, 2008: 255–260). Also in several regions in Lapland to the north and northeast of the Bothnian Arc, a lack of prehistoric pottery and semi-subterranean dwellings has previously been noted (e.g. Halinen, 2005; Oksala, 2009).

Along the Northeast Bothnian "fishhook", predominantly local building forms emerged. These include row houses connected by shared corridors (see Pesonen, 2002: 27–28; Mökkönen, 2008: 131–137) and, in the southern half, giant's churches. Giant's churches are typically large, up to 70 x 40 meters, rectangular or oval enclosures with 2–3 m wide, low stone or boulder walls. The nature of the remnants remains somewhat debated, but the emerging consensus is that they belong mainly to the Middle Neolithic of the 3rd millennium BCE (see Okkonen, 2014 for an overview). There are also indications that the cairn burial tradition began around the same time (Forsberg, 1999; Okkonen, 2003; Núñez & Okkonen, 2005), although this remains unverified. It is also a possibility that stone settings and cairns had been in continuous use from the Late Mesolithic, from the times of the red ochre stone burials, but if so, this remains concealed due to lack of research.

In any case, significant changes took place yet again throughout the region around 2900–2800 BCE. An archaeological phenomenon known as the Corded Ware Culture (see Edenmo, 2008; Nordqvist & Häkälä, 2014; Beckerman, 2015) spread to Southern and Southwestern Finland, followed apparently either simultaneously or shortly after by an expansion to Southern and Eastern Sweden (Gustafsson, 2007; Pesonen, Larsson, & Holmqvist, 2019).

Having ultimately expanded northward up to Kokkola on the Kvarken, the "Corded Ware frontier" settled (see Fig. 11; Äyräpää, 1937: 116–120; Nordqvist & Häkälä, 2014), with lesser influence further up north, mostly in the form of single artefact finds (see e.g. Carpelan, 2004b). The northernmost burial related to Corded Ware Culture, in Kuoppakangas [483010016], is on the southeast coast of the Bothnian Arc, just 100 km southwest of the Oulujoki River. Similarly, the extent of

Corded Ware influence, manifested in multiple pottery finds, reached the Byskeälven River on the western coast of the Arc (Knutsson, 1988). The culture, with its roots possibly from as far as the European steppe region (see e.g. Anthony, 2007: 367–368; Malmström et al., 2019), was heavily invested in animal husbandry, and seems to have introduced goats and sheep to Central Fennoscandia (e.g. Cramp et al., 2014; Beckerman, 2015: 21–22).

Fig. 12. Boat axes in the now defunct prehistory exhibit of the Finnish National Museum (photo by author).

The Finnish Corded Ware Culture, also known as the Battle Axe Culture, is currently interpreted as predominantly pastoral with little evidence of staple food crops (see Lavento, 2012a: 5–7; Cramp et al., 2014; Vanhanen, 2019). Even though not all the related communities maintained farming practices, Corded Ware Culture can be seen as transmitting a form of production ideology (Chapter 6.1; Paper III; see also Cunliffe, 2008: 167–169). In Central Fennoscandia, such an ideology had no clear precedent. Nevertheless, studies of animal bones excavated in the CWC dwelling sites indicate that these communities still relied greatly on foraging, and especially fishing and seal hunting (Bläuer & Kantanen, 2013: 1655).

By 2300 BCE the culture had transformed into composite regional identities, fusing with local cultures (Kivikoski, 1961: 71–72; Lavento, 2012b: 148; Holmblad, 2013: 85–86). On the western coast of the Bothnian Sea, the site of Hedningahällan [Enånger 68:1], an activity site containing 130 kg of pottery sherds of four different contemporary styles, including Corded Ware, and also grains of barley, and a

considerable amount of seal bones, suggests admixture of different local cultures (Stilborg & Holm, 2009). On the other hand, around the Gulf of Finland, material expressions of the local Corded Ware Culture may have remained largely unchanged until the beginning of the Bronze Age (Nordqvist, 2016).

The distributions of both the giant's churches and the Corded Ware Culture intersect at the east coast of the Kvarken, within a 100 km long coastal strip where both seem to have been present around the same time. No giant's churches have been found on the western or northern coasts of the gulf. The absence of Baltic amber, beginning from the Middle Neolithic in the regions north of the Corded Ware zone, suggests that some material networks were cut off and rerouted at this time. Also the use of Bothnic tools may have begun to wane in regions outside the Tervola region of the Kemijoki River valley, with only sporadic finds encountered during the 2nd millennium BCE (Paper V; see also Rantala, 2014). On the other hand, some segments of the northern network seem to have intensified, as indicated by the increased circulation of quartzite and asbestos in the forager region (Baudou, 1992: 109–113; Lavento, 2001: 128–132; Welinder, 2009: 320, 327, 329). The interlocking distribution areas on the east coast have led to speculation about the role of warfare between the two cultures (see Sipilä & Lahelma, 2007). Giant's churches could well have functioned as stone foundations for palisades, meant for keeping out intruders.

While no physical proof of violence has been found, the situation can be interpreted symbolically. This way the polished axes, so-called boat or battle axes (see Fig. 12), of the Corded Ware Culture represent belligerence while the giant's churches represent defensive refuge. It is easy to see why especially the "battle axes" have influenced many archaeologists in interpreting a conflict (e.g. Kivikoski, 1961: 167–169; Huurre, 1998: 55; Sipilä & Lahelma, 2007; see also Beckerman, 2015: 25). Similar symbolic resistance has been deduced from the exaggerated size of the Pyheensilta arrowheads, which are interpreted to originate from the Satakunta region east of the Bothnian Sea, a material expression, which may have also been contemporaneous with the early Corded Ware Culture (Salo, 1997: 46).

The symbolism probably did not go unnoticed to the contemporary communities either. The conflict may have been a real one, as often is when differing ideologies and worldviews collide. Even so, the likeliest scenario is that relationships between the groups were decided locally and went through complex motions throughout the centuries. Both the Corded Ware Culture and the use of giant's churches may have lasted in the region for more than half a millennia, the

former merging with local material cultures to form the foundation for Bronze Age agricultural communities, with the latter tradition ending shortly after (Paper III).

5.2 Coexistence and intensification of networks from the 2nd millennium BCE to an abrupt gap in the 1st millennium CE

The post-Corded Ware composite cultures of the south and their northern forager neighbors continued their coexistence. Gradually the northern foragers abandoned the use of semi-subterranean dwellings, favoring other less visible dwelling forms, contributing to a dramatic decrease of archaeological data, often interpreted as population decline (e.g. Okkonen, 2003: 231; Tallavaara, Pesonen, & Oinonen, 2010; Lavento, 2012a: 29; cf. Chapter 9.4). According to shoreline displacement chronology (Paper II), the last two probable Neolithic villages with several semi-subterranean dwellings, Morruttajankangas [429010018] and Yli-Harjunselkä 1 [1000011858], were built sometime after 2300 BCE, with the former established after 2000 BCE. Single semi-subterranean dwellings remained in use at least until 1400 BCE (Paper II; Norberg, 2008).

Apparently herding remained an important subsistence mode in the south. The first concrete evidence of domesticated animals in the region is a sheep bone dated to 2200–1950 BCE, found at a late or post-CWC dwelling site in Pedersöre (Bläuer and Kantanen, 2013). Later Bronze Age domestic animal bones have been found in the southeast coast of the Bothnian Sea, dating from 800 BCE to 300 CE (Bläuer & Kantanen, 2013: 1649, Table 1). Also, a recent date from the burnt bone of either a goat or a sheep buried in a stone setting [Hortlax 73:1] just 24 km northeast of the Byskeälven River, indicated a date of 900–800 BCE (2697±31 BP, Ua-53993, Palmbo, 2020 personal communication). Either the raising of livestock was practiced in the Bothnian Arc, or this particular animal originated from further south.

Bronze began spreading rapidly throughout Fennoscandia around 1800–1500 BCE. One of the northern region's earliest sites with bronze objects, containing both an eastern dagger or a spearhead and the flanges of a broken Southern Scandinavian blade (Ikäheimo, 2019; cf. Forss & Tuovinen, 2001: 9–10), is the cremation burial of Hangaskangas [564010051] along the Oulujoki River. The cremation dates to as early as 1900–1600 BCE, although this dating is based on a single radiocarbon dated sample (Okkonen, 2003: 231; Ikäheimo, 2005: 780; Paper II).

Fig. 13. The massive "long cairn of Huilu" in Sammallahdenmäki, a UNESCO Heritage Site, located on the eastern coast of the contemporary Bothnian Sea (photo by author).

Two influxes of metal seem to have occurred contemporaneously: the spread of eastern bronze axes throughout Finland (Lavento, 2001: 120–121; Lavento, 2012b: 151–153; Chernykh, 2012: 115, Fig. 5) and the spread of Central and Western European bronze objects to Southern and Central Sweden, Southern Norway, and Southern Finland (e.g. Huurre, 1979: 97; Larsson, 1986). Famously, four Southern Scandinavian bronze swords, "the swords of Petkula" dating to ca. 900–700 BCE, were found in the early 20th century in Central Lapland, having travelled more than 1,000 km from their place of production to their eventual site of deposition (see Tallgren, 1907; Lavento, 2015: 179). These material networks seem to have remained in action during the whole Bronze Age of 1700–300 BCE, as we will later note regarding socketed celt-axes.

As noted before, the tradition of constructing stone burials was common in Southern Sweden already during the 4th millennium BCE (Larsson, 2014; Blank et al., 2020). In the Bothnian Gulf region cairns were built along with giant's churches in the 3rd millennium BCE, although there is yet no evidence of them being used

as burials (Okkonen, 2003). The burial cairn tradition in Central Fennoscandia began at the latest around 1800–1400 BCE in the form of long-cairns. These were used throughout the Gulf of Bothnia (Okkonen, 2003: 111–112), and also in Finnish Lakeland (Saipio, 2015).

At the beginning of the Bronze Age the post-Corded Ware regions in Central Fennoscandia also embraced cairn burial. Around 1500 BCE the tradition was adopted in Southwestern Finland, where compared with the rest of the gulf it was taken to an extreme (see Asplund, 2008). The massive burial constructions of Satakunta (see e.g. Salo, 1984: 127–134), most famously the UNESCO world heritage site of Sammallahdenmäki (Fig. 13; see also Tuppurainen, 2014), exemplify the difference between the former Corded Ware region and the forager region, the latter's cairns being overall much less prominent (Paper III; see also Saipio, 2015).

Beginning from the Early Bronze Age, cremation burials were practiced alongside the more traditional inhumation. Several burial customs were shared in many places on the coasts of the Baltic Sea, such as tarand-graves (Lang, 2007: 104) and stone ship setting burials (Wehlin, 2013). Thus burials speak of a world connected by the sea yet distinguished by various local traditions (Paper IV).

Related to the innovations of metallurgy and the manipulation of fire in cremations may be the aggregated use of cooking pits in Central Fennoscandia and Southern Scandinavia, where several sites contain hundreds of such pits. The clusters are often interpreted as signs of surplus production, related to cult activities (Gustafson, Heibreen, & Martens, 2005), or feasting (Kuusela, 2014a). These sites were used mostly from 1000 BCE to around 500 CE, although the exact dates vary by region.

In Southern Sweden, the largest cooking pit clusters were used around 1000–600 BCE (Martens, 2005) and in Denmark ca. 1000–100 BCE (Henriksen, 2005). The radiocarbon dated cooking pit clusters of the eastern Bothnian Gulf coast date mainly to 800–200 BCE (Kuusela, 2014a; Paper II). In the Lake Mälaren region, and to a lesser extent in the Åland islands, instead of cooking pits, mounds of burnt rocks (Swe. *skärvstenshögar*) clustered together date to a longer period from 2000 to 800 BCE (Welinder, 2009: 294–295). The widely spread large-scale use of cooking pits, along with the contemporary developments in maritime travel (Wehlin, 2013), suggest that the communities of the Bothnian Arc were in regular contact with the long-since established farming communities of Southern Scandinavia (see Fig. 14; see also Kuusela, 2013).

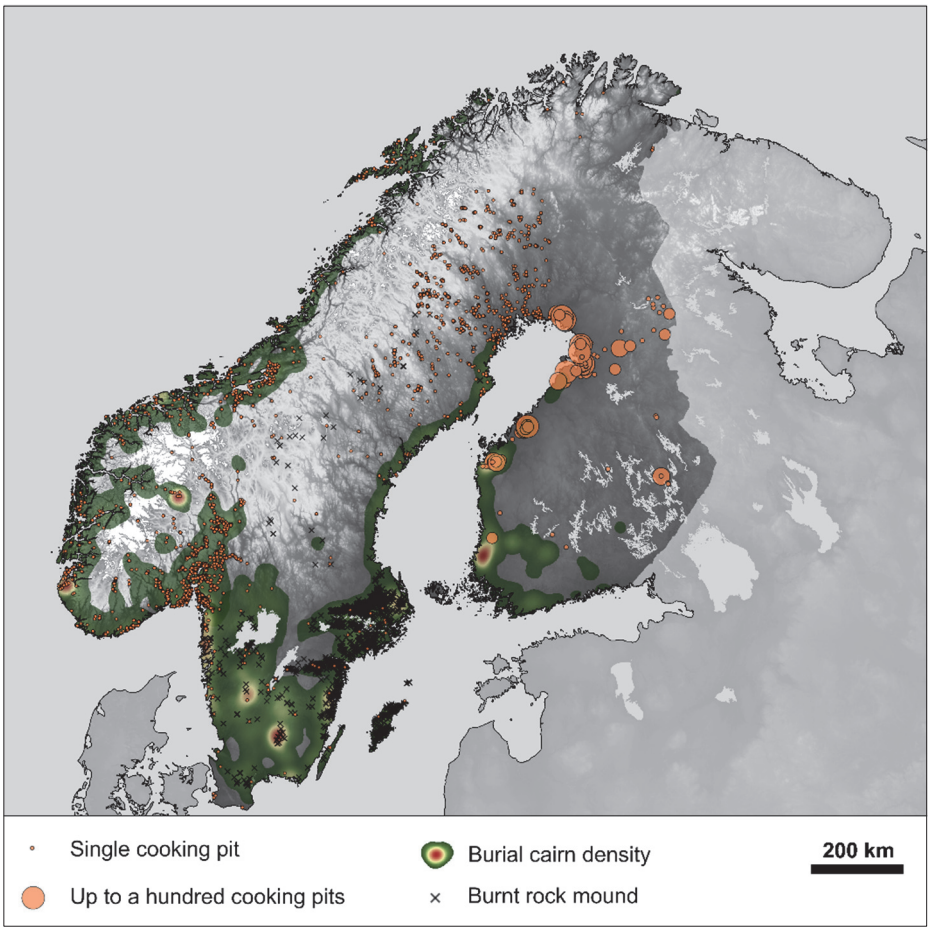

Fig. 14. Distribution map of the main material indicators of the Fennoscandian Bronze Age and Early Iron Age. The representation of cooking pit sites differs due to differences in the national archaeological site registries. DEM from DIVA-GIS.

Fig. 15. The extent of the Northern European-wide technological complex of the Late Bronze Age socketed celt-axe, shaded. The three main regions of axe production are A. South England, B. Lake Mälaren, and C. Volga-Kama confluence. Tilted lines indicates the Late Iron Age expansion of the Norse, suggesting corresponding maritime and riverine routes between east and west.

In the larger context, at least two major material networks extended throughout much of Northern Europe, one longitudinally and the other latitudinally. The first network, extending from the Baltic Sea to the partly urbanized state societies of the Mediterranean, is evident in the movement of southern bronze and copper to the north and northern amber to the south, along with tin from the British Isles and

Central Europe (e.g. Ling et al., 2014). The other network is exemplified around 800–500 BCE, by the distribution of socketed celt-axes. These axes are clearly technologically associated, tracing a network stretching throughout most of Northeastern and Northwestern Europe from the Kama region of Tatarstan in Russia all the way to the British Isles, Southern France, and even Northern Spain (see Chernykh, 1992: Fig. 84–87, 89; Huth, 2000: Fig. 12.1–12.4; Maraszek, 2000: Fig. 14.1 and 14.6; Yushkova, 2012: Fig. 7.3–7.5; Roberts et al., 2015; Fig. 15). The socketed celt-axe technology may even extend as far as China (see Zhang, 2006: Fig. 7; Mei, Xu, Chen, Shen, & Wang, 2012), but apparently not to the Mediterranean region. The vast openness and contact-extensiveness of the Eurasian world between 1700–300 BCE, and especially the Axial Age of the later half, has been recently discussed by many archaeologists and historians (see e.g. Graeber, 2011; Kuusela, 2013; Cline, 2014; Cunliffe, 2015; Scott, 2016).

Despite the vastness and arguably even globalism of the Bronze Age world, a distinction between two traditions of cooking pit use can be seen even on the coast of the Bothnian Gulf. During 800–200 BCE, burial cairns and cooking pit clusters on the eastern coast, from Pedersöre to Tornio, were mostly connected to river estuaries. Thus it is likely that the related communities were heavily dependent on these locations, indicating mainly foraging lifestyles. The combination of foraging subsistence with surplus production activities indicates an admixture of ideologies related both to procurement and production (see Paper III; also Chapter 6.1). Contrastingly at Laihia on the eastern Kvarken, the Bronze and Iron Age cooking pits, cairns, and dwelling sites were located closer to meadows and marshes than to contemporary rivers or the coast, implying that agriculture and pastoralism dictated the location where the communities were maintained (Holmblad, 2010: 110–111). This observation suggests that production overrode procurement as an ideology (Paper III; also Chapter 6.1).

At the verified onset of the cairn burial tradition in the early 2nd millennium BCE, cairns were spread quite evenly along the coast of the Bothnian Gulf. However, during the Bronze and Early Iron Age, the northern limit of over 1 m high large cairns receded to the south (Okkonen, 1998b; Forsberg, 1999: 258; also Paper III). According to the distribution of cairn sites at different elevations on the Finnish coast, Early Bronze Age cairns were built in large clusters up to Pyhäjoki, the region formerly influenced by the Corded Ware Culture. Instead, in the northern forager zone during the Bronze Age, cairns were mostly low single structures, in contrast with the large, clustered cairns in the south.

Fig. 16. A probable Late Bronze Age burial stone setting at Tahkokangas in Oulu, deturfed in 2011. The tree in the background is 30 cm in diameter (photo by author).

Later, during the Early Iron Age, defined here as 300 BCE to 200 CE, large coastal cairns were built up to the latitude of Vaasa, while further north, instead of heaped cairns, mainly inconspicuous stone settings with only a few layers of stones were built. The final phase of northern cairns, beginning around 200 CE, saw a change to more prominent but still comparatively small burial structures with an increase in grave goods. In the north, the cairn burial tradition seems to have ended completely around 500 CE (Paper III; see also Okkonen, 2003). On the Swedish coast, prehistoric cairns are numerous up to Piteå and recede south in a similar chronological fashion than in Finland (see Forsberg, 1999: 258), and, with two exceptions along the Sangisälven River, the distribution of Iron Age earthen burial mounds reaches only the region of Örnskjöldsvik (see Fig. 18; Ramqvist & Hörnberg, 2015: 122).

Resource management changed throughout Fennoscandia with the introduction of iron. No longer was access to metal dependent solely on long-distance networks. Bog and lake ore allowed iron to be acquired and worked locally.

Still, the knowledge of iron working spread unevenly. Communities of the Lake Mälaren region of Sweden may have begun processing iron as early as 1200 BCE (Stenvik, 2003: 126; Welinder, 2009: 39). This date is suspiciously early, and the discrepancy could be caused by uncertainties in radiocarbon dating, possibly a systematic error in the dating method (see Gassmann & Schäfer, 2018). In contrast, the earliest agreed-upon evidence of iron production in Germany dates to the 6th century BCE (Gassmann & Schäfer, 2014: 22). The earliest imported iron objects on the Arctic coast have been interpreted to date as early as 800–500 BCE (Olsen, 1991: 74; Sundquist, 1999).

Fig. 17. A huge Bronze Age burial cairn at Kylmäkorvenkallio [684010037] near Rauma east of the Bothnian Sea. On the right, as scale, anthropologist Jenni Ahto-Hakonen (photo by author).

Gradually iron became more widely used in Fennoscandia, largely replacing bronze as a material for tools and weapons sometime after 500 BCE. I use 300 BCE as the local limit between the Bronze and Iron Age, instead of the traditional 500 BCE, since the former seems better fitting based on the use of cooking pit clusters, which are relevant due to their hypothetical role in accessing bronze (see Kuusela, 2013; also Kuusela & Okkonen, 2013). Also, most of the regional evidence for the adoption of iron and the emergence of its local production occurs ca. 300 BCE.

There is nevertheless comparatively little evidence of iron production in Finland and Northern Sweden during the Early and Mid-Iron Age. Less than ten

stone box furnaces, structures for the smelting of iron ore, have been found throughout the region (Karjalainen, 2016: 8–9). In contemporary Estonia, timber-framed furnaces were used, while in Latvia and Lithuania base-stoned furnaces were favored (Peets, 2003: 287). There seem to have been widely varying experimentations regarding iron production. Early sites related to iron production on the Bothnian Arc are Nordmaling 648:1, dating to ca. 100 BCE (Andersson & Sandén, 2007), and Rakanmäki [851010002] dating to 200–400 CE (Mäkivuoti, 1988). At Rakanmäki, at the northern end of the Baltic Sea, a Norrlandic spade-shaped iron bar was found, bearing the style of the iron production taking place in Jämtland (see Lindeberg, 2009).

Contrastingly to the small-scale production in the aforementioned regions, the mountainous regions of Southern Scandinavia, with its shaft furnaces, technology similar to that used in the Roman Empire, became the epicenter of preindustrial iron production in Northern Europe (Espelund, 2014**;** Larsen & Rundberget, 2014). In Central Fennoscandia iron production was the most intensive in Jämtland, in the region adjacent to the western mountains (Lindeberg, 2009), and in Gästrikland (Hjärthner-Holdar, Forenius, & Willim, 2014) on the southwest coast of the Bothnian Sea. Research of Iron Age Jämtland by archaeologist Marta Lindeberg (2009) suggests that the iron bars were originally produced mainly for ritualistic purposes as sacrificial objects, but outside the core distribution area they were increasingly used as raw material for producing objects such as iron kettles (also Lindeberg, 2010). The iron bars were produced from 100 to 900 CE (Lindeberg, 2009).

The southern zone is represented by numerous sword finds, which are weapons of both physical violence and symbolic power. Contrary to most other bladed objects, swords are bad hunting weapons and even worse tools, signifying human-on-human force. In Southern Sweden swords were used beginning from the very early Bronze Age (Larsson, 1986), but they became numerous in Southern Finland only during the Iron Age (Moilanen, 2015). From 400 CE onwards the number of deposited swords increased (Moilanen, 2015: 18–19). On the Swedish coast the role of violence is made evident by the site of Högom [Selånger 1:1] in Sundsvall, dated to 400–550 CE, where the remnants of a burnt down long-house, inside of which were the bones of up to three women, lay under a burial mound (Ramqvist, 2016).

In Finland, cremation cemeteries are often interpreted to indicate the extent of the Iron Age farming communities (e.g. Wessman, 2010), where swords were also relatively numerous. The burial type is predominant in the areas surrounding the

Gulf of Finland in the southeast, and nearly non-existent in Central Fennoscandia, except on the Finnish coast up to Vaasa. In Sweden, the contemporary northern limit of long-houses, hill forts, and burial mounds related to agriculture was near Örnsköldsvik (Ramqvist & Hörnberg, 2015: 122), opposite Vaasa.

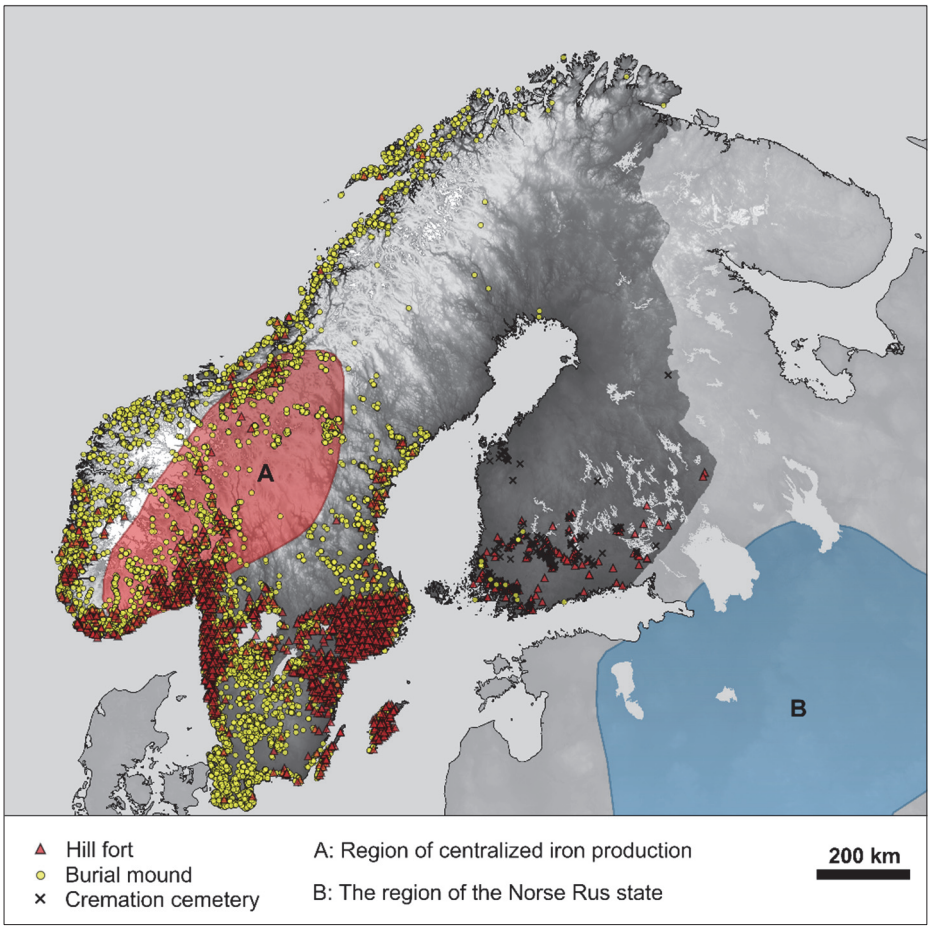

Fig. 18. Distribution map of the most apparent Later Fennoscandian Iron Age material indicators. Note that the regions northeast of the main distribution of the exhibited indicators are mostly blank, indicating the contemporary forager zone. The archaeological records of these regions are fragmented, with no cohesive remnant-type indicators unique to this period. In these regions the bulk of the contemporary archaeology is represented by stray finds (see e.g. Hakamäki, 2018), which have not been collated here. DEM from DIVA-GIS.

Välikangas [564010022] near the Oulujoki River contains some of the few cairns on the northeast coast of the gulf that include weapons. Two swords, a seax, five spearheads, and two axes were found here along with 12 burials (Mäkivuoti, 1996: 55–68), the largest weapon stash found in the region, implying the influence of a hierarchy making use of at least symbolic violence.

After ca. 400–500 CE burial cairns were no longer constructed in the northern forager zone, initiating a significant gap in the archaeological material record of the coastal Kvarken and Bothnian Arc, occasionally referred to here as the "Mid-Iron Age forager gap". The gap roughly coincides with the end of the use of stone tools as well as ceramics, which seem to have been replaced due to increased circulation of metals (see Bergman, 2007). The volume of iron production, especially in Southern Norway, and the extent of the material network due to advanced forms of maritime travel may have made the use of local and relatively cumbersome resources largely obsolete. Both the reuse of copper and bronze and the corrosion of iron may have contributed to the imperceptibility of the archaeological record between ca. 400–800 CE (see also Bergman, 2007). On the western Kvarken coast, extending to the western Bothnian Arc, the use of cooking pits resurged, along with an increased use of hunting and fishing base camps (see e.g. Broadbent, 2010; Bergman & Ramqvist, 2018). Also in the same region, some forager communities may have begun experimenting with agriculture (Bergman & Hörnberg, 2015).

In the regions surrounding the Bothnian Sea and other southern agricultural regions, no similar gap in the material record exists. Instead, large cairns and burial mounds continue to indicate hierarchical relations. On the other hand, the archaeological record of 600–800 CE slightly declines also in the regions northeast and west of the Bothnian Sea (Liedgren, 1983; Ramqvist, 2012). Still, the use of large mortuary sites east of the Bothnian Sea seems to have continued unabated through the Middle Iron Age (Salmio, 1980; Lehtosalo-Hilander, 2000).

The Late Iron Age sees the final prehistoric phase, which preludes the establishment of states (see e.g. Hedeager, 1992; Thurston, 2002). During this period, the Viking Age of 800–1025 CE, the number of swords and other weapons in the archaeological record peaks (Moilanen, 2015: 18–19). Swords are often interpreted, in addition to the previously discussed specialized use for violence, as expressions of hierarchy and status (Moilanen, 2015: 322–323). Only a few Iron Age swords have been found in the northern forager regions (e.g. Hakamäki, Hakonen, Moilanen, & Kuusela, 2013; Hakamäki & Kuusela, 2013; Hakamäki, 2018: 37), compared to the hundreds in Southern Finland alone (see Moilanen, 2015: 16–20).

This contrast also corresponds with the establishment of hill forts throughout most of the agricultural zone—national registries indicate, with a high error margin, 120 sites in Southern Finland, 1,300 in Sweden, and 390 in Southern Norway—illuminating a part of the network of violence that plagued Northern Europe at the time (Haywood, 2015). While no similar defensive structures have been found in the northern forager zone, the over 50 battle-axes and more than a dozen spearheads and arrowheads found in Northern Ostrobothnia and Kainuu, to the east of the Bothnian Bay, indicate that the forager regions were not isolated from the contemporary events (see Hakamäki, 2018: 36–41).

After 800 CE the archaeological record of the northern forager zone begins to recuperate. The Late Iron Age in the region has only recently come to light (see e.g. Hakamäki, 2018). There is yet no evidence of significant agriculture or pastoralism displacing the forager regions until well into the Historical Age, beginning from 1300 CE, but future research will hopefully shed more light on this issue. The domestication of reindeer may have begun around 800 CE in the western mountain regions, but reindeer pastoralism became a major form of subsistence only after the 15th century CE (see Bergman, Zackrisson, & Liedgren, 2013; Bjørklund, 2013; Salmi, Äikäs, Spangen, Fjellström, & Mulk, 2018).

The distribution of medieval Christian churches and Sámi sacred sites indicates the extent of the two cultures as it remained for much of the 2nd millennium CE (Fig. 19). The shared dynamic of the cultures during the medieval period lies mostly outside the scope of this study, but the reader is referred to recent illuminating studies (e.g. Kuusela, Nurmi, & Hakamäki, 2016; Bergman & Edlund, 2016; Bergman & Ramqvist, 2017; Bergman & Ramqvist, 2018; Kuusela, Nurmi, & Hakamäki, 2018; Kuusela, Salmi, & Äikäs, 2020; Nurmi, Kuusela, & Hakamäki, 2020).

We will conclude this overview of the region's prehistory to this point in time. The most notable observation in this large-scale narrative is that the cultural divergence which still exists in Fennoscandia can be traced far back into prehistory, at least to the Middle Neolithic. Furthermore, I posit that should cultural binarism be applied in developing such narratives, the pertinent binary is not Finnish and Swedish nor Sámi, but the ideological contexts of *procurement* and *production*. In the next chapter, we will turn our attention to this argument.

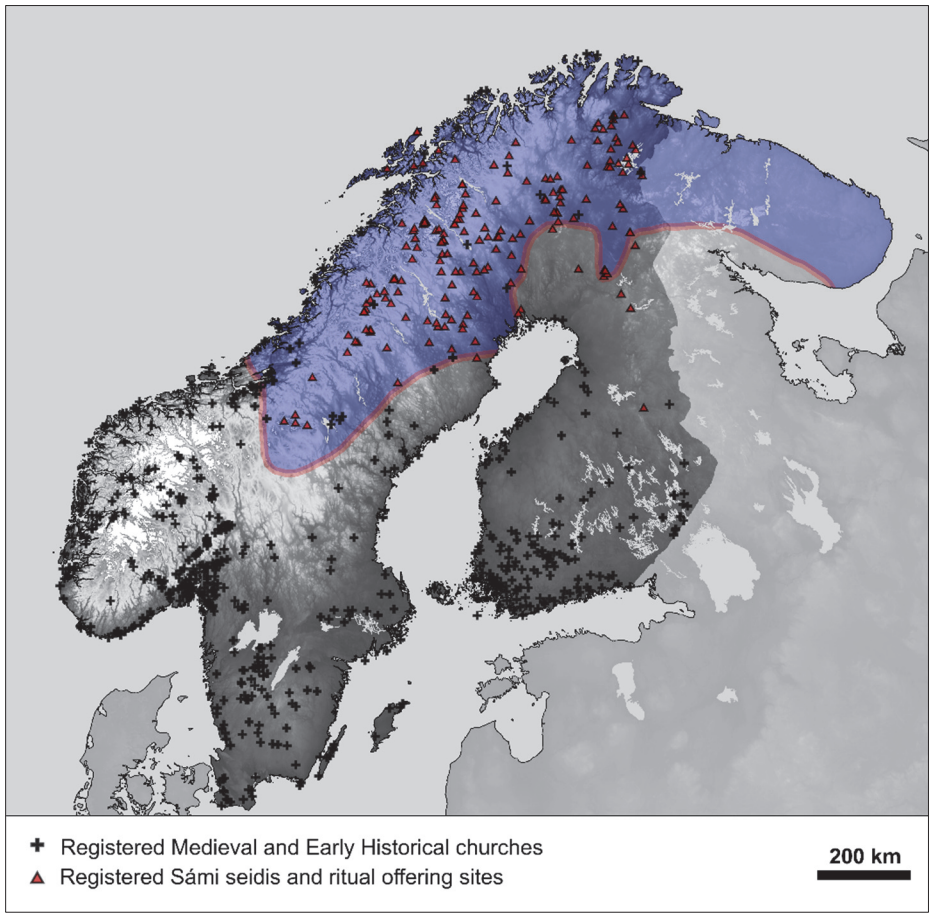

Fig. 19. Distribution map of Christian churches and Sámi sacred sites, showing the geographic separation of the worlds. The shading indicates the modern indigenous region of Sápmi or Sámiland. DEM from DIVA-GIS.

6 Analysis: Life and death in prehistoric Central Fennoscandia

In this chapter we will change our perspective from reviewing prior research to interpreting ideological and cosmological issues beyond the material constraints. This chapter is based on Papers III and IV, with an added emphasis on commentary on their conclusions. The analyses are presented more thoroughly and with more relevant citations in the original research papers.

Our focus is two-part. The first part is ideological. This includes matters of wider cosmology, the understanding of one's place in the surrounding world, and the narrower concept of politics, or how and to what end groups should be organized. The second part is empathetic, more precisely concerned with the emotional response to death. The range of responses is vast, whether bereavement, reverence, fear, or any of the sort. By contemplating what all these emotions share, I argue that the theme which connects practically all mortuary sites and practices is—unsurprisingly to most yet perhaps controversially for some—respect.

These elements combined may give us a better understanding of life and death in prehistoric Central Fennoscandia; an understanding, which I hope to have formulated with a durable epistemological lineage.

6.1 Subsistence ideology, labor, and power relations (Paper III)

With the narrative of Central Fennoscandian prehistory in Chapter 5 acting as our material constraints along with the context presented in Chapter 4, and the theoretical toolkit of Chapter 3 finally guiding our thought in earnest, we may take an admittedly risky foray into the archaeology of thought and relations. Such a move invites the danger of prescribing current moral thought into wholly unrelated matters, doing disservice to our mute respondents. Then again, history as a discipline was founded on the study of the ideological, which makes the line between the archaeological and the historical records stand out as the proliferator of thought. Without taking the risks of proceeding into the unknown beyond materialism, archaeology is restricted either to the role of antiquarianism, with its relevance in the fascination with curios, or to structural processualism, projecting strategic progression to the far past. While both perspectives have their merits, some of which I have adopted in Paper III and this chapter, there undoubtedly remain new and open ways to explore elements of materiality as ideological manifestations.

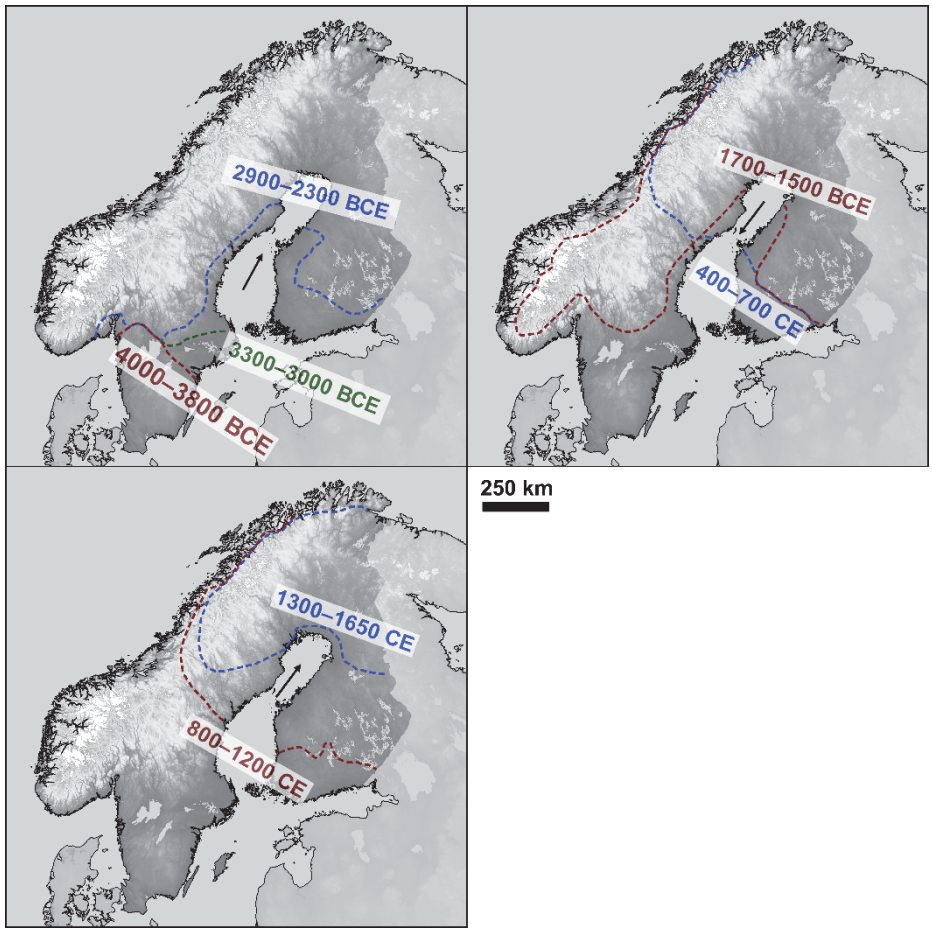

Fig. 20. The conceptual frontier between the two subsistence ideologies in Fennoscandia and its movement throughout the past. Based on the overview in Chapter 5. The maps are reproduced from Paper III.

In Paper III, I have argued that a cultural divergence emerged with the arrival of agricultural practices in Southern Scandinavia. A millennium later, similar practices in the form of pastoralism spread to Central Fennoscandia, bringing to the region a separation between two subsistence ideological forms: the northern forager-related ideology of procurement and the southern agriculture-related ideology of

production. The former had existed in Fennoscandia throughout the Mesolithic, but it gains more relevance in research with the contrast to the latter ideology.

The advantage of expanding the issue of subsistence from its mere practice to the ideological is that it is then no longer an issue of mere cause-and-effect. Instead, the matter becomes a complex entanglement taking place both in the material realm and the worldview of the individual, and relationships between them. It matters less whether the actual living members of the archaeological Corded Ware Culture practiced foraging, pastoralism, or farming, or whether plant cultivation was experimented with in Late Mesolithic. What matters more is how these practices affected the worldview and ideology, or how other seemingly unassociated practices conformed to a worldview relatable to the enacting communities. In this theoretical formulation, subsistence farmers are able to live by procuring their livelihood through co-action with plants and animals, while a forager community may maximize its surplus food production, without instantly breaching the limits of their designated categories.

In subsistence ideology, the matter concerns how one justifies their own maintained existence. Conceptually, the ideology of procurement maintains that sustenance is given by the environment and is therefore not strictly owned; while in the ideology of production, sustenance is brought about by the work of the consuming entity, making the product theirs by default. This conceptualization is merely the extreme binary or polar opposite form, and a "pure form" is doubtful to exist in most cases. Still, by comparing the two different material cultural contexts of Fennoscandian prehistory, we can see this dynamic in the contrast between the material contexts. The two subsistence ideological trajectories lead to different actions in the political realm, which may be observed in many instances within the archaeological record, but none so vividly as with issues of extra-subsistence labor.

In the Mesolithic record, labor practices are by all indication subsistence related. Stone tools, and surviving examples of woodworking from earlier or contemporary yet distant places (see e.g. Lozovskaya & Lozovski, 2016), reveal an acute understanding of the worked materials, yet no clear prerequisite for labor specialization. Works of intense labor are yet absent from the record. Pottery production shows no signs of being any more intensive than occasional communal work. The Early Neolithic sees a substantial change in housing, with semi-subterranean dwellings being erected in the thousands. But what makes the dwelling record so transparent is simply the depressions on the ground and its shallow surrounding embankment. If the dwelling were not semi-subterranean, we would probably not recognize it. Except in a few cases, where the lower timber

frames have been preserved through burning, of the walls and roof, nothing remains. Thus, we do not truly know how similar the structures or in fact their numbers are in relation to the preceding dwelling tradition.

Once the semi-subterranean aspect of dwellings falls out of fashion during the Early Bronze Age, dwellings all but disappear from the northern archaeological record, not to be explicitly seen again until stone ovens of the Late Iron Age and Church Period ignite a new archaeological dwelling marker. In a short-lived retro-phase, dated to the Late Bronze Age or Early Iron Age (see e.g. Andersson & Sandén, 2007), semi-subterranean dwellings saw renewed sporadic use. Only a few possible examples have been discovered, for instance the unexcavated site of Isokankaan reuna [436010009] with three suspected dwelling depressions. Mainly though, in the producer record of the south, housing is indicated by the arrangement of post-holes, made visible only through large-scale excavations. Such houses can generally be found if they are near highly visible burial monuments, or else by sheer luck.

Thus, when the northern archeological record diminishes and practically disappears after ca. 400 CE—the simplified "Mid-Iron Age forager gap"—we should first juxtapose the evidence with the Mesolithic record, which can be tentatively seen as a sort of baseline (with due caution, see Wengrow & Graeber, 2015; Hakonen, 2021), before inferring an absence of people. So what are the similarities? First, the seeming absence of extra-subsistence labor; second, the unsurprising absence of dwelling remnants, unsurprising, since that is the norm if there are no dwelling depressions; third, a near-invisible mortuary record, with the difference that during the Late Mesolithic burial visibility increased slightly due to luck (e.g. the discovery of Tainiaro, see Hakonen, 2019b) and systematic work of archaeologists (red-ochre stone setting burials, see Liedgren, 2014).

The dissimilarities include the absence of stone tools (except strike-a-lights) and debitage and the absence of pottery. Both of these developments seem to have taken place a century or two prior to the "forager gap", as indicated by the materiality of the few excavated sites. These include the activity site of Rakanmäki [851010002] and the burial cairns of Välikangas [564010022], which were both discovered due to the high visibility of burials cairns, and also in the case of Rakanmäki the fortunate timing of furrowing. In Rakanmäki, the find assemblage consists mainly of burnt rocks, nearly 5 kg of slag, and more than a dozen metal objects, with less than 30 quartz flakes and also 60 quartzite flake, the latter possibly fragments of strike-a-light stones (KM 24206:1-10, KM 24207:1-41, KM 24208:1-122, and KM 24209:1-95). Välikangas, in addition to numerous metal

artefacts, contained only a quartzite strike-a-light, a few potsherds, and two quartz pebbles (KM 23911:1-35 and KM 24597:1-48).

It would seem that both stone tools and pottery became obsolete at the same time due to the increased supply of iron (see also Bergman, 2007), first from the western inland and coastal regions of the Bothnian Sea and after that from the mountain regions of Southwest Scandinavia. Thus, the main difference, as I see it, between the Mesolithic and the Mid-Iron Age archaeological record in the forager zone is the origin and inherent chemical preservation of artefacts.

I argue, elaborating on the contention of Jari-Matti Kuusela (2013), according to whom the deficit of Mid-Iron Age materiality represents established power relations, that the two periods were high points of political anarchism, when egalitarianism was accentuated with minimal symbolic representation of status. In these periods, according to this perspective, practicality mattered the most, as well as minimizing the unnecessary labor of everyday life. Early Neolithic seems to have been highly similar in this sense, except that the use of exotic ornaments, such as Baltic amber, may indicate more status-oriented habits.

After the beginning of the Middle Neolithic, the communities represented by the archaeological Corded Ware Culture not only brought with them domesticated animals but also a novel ideological relationship with the environment. What mattered, in subsistence and in symbolic representations, i.e. the finely and laboriously crafted boat axes, was production. According to this interpretation, this is when subsistence ideologies diverged, consolidating the existence of two main cultural contexts. Although perhaps representing a more moderate version of the ideology of production than in later periods, the cultural divergence seems to have been evident from the start. This is so even without considering the tentative results of genetic studies of burial remains from the CWC context, suggesting ancestral lineages from the European steppe (e.g. Malmström et al., 2019).

In the production ideological context, matters of life and death of domesticated animals were dictated by humans, instead of subsistence being provided by the will of the waters and forests (see e.g. Bird-David, 1990). How the producers saw themselves, or how they were seen by others, are questions with answers that are mostly unverifiable. One can see them as subjugators of animals, who graze on engineered meadows, with ownership yoking both life and land to the whims of humans. Less cynically, the communities may have seen themselves as caretakers of their animals, in what may be regarded as a form of social contract (see e.g. Armstrong Oma, 2010; 2018). Specifically those seeing the world in terms of procurement could have perceived the contrast in power relations between humans

and animals as an immorality. When the procurers may have disregarded the ideology of owning living beings and procured some of the animals offered by the tended meadows, conflict would have undoubtedly ensued.

This conflict may be expressed by the battle or boat axe. This is not a practical tool or even necessarily a practical weapon. Its importance is by all understanding symbolic. It differs from anything that was seen in the region previously. The material, mainly diabase and olivine diabase, fragments unpredictably. There are few sites with diabase flakes and refuse, which is at odds with the over a thousand boat axes found throughout Finland and Sweden. My own experiments with the material have led me to believe that boat axe preforms, which through grinding and polishing achieved their final form, were shaped not so much by knapping, but with studious pecking. Pecking reduces the worked surface slowly but surely, minimizing the possibility of mishaps, while leaving only dust instead of flakes as the invisible debitage. The shape and contouring of boat axes are elaborate, often with hard protruding edges around the shaft-hole, and sometimes with simulated decorative cast-seams, made by careful polishing instead of fast grinding. Still, the most time-consuming effort is the drilling of the shaft-hole.

Shaft-holed stone axes are not known in the region prior to the Corded Ware Culture, perhaps because they are simply impractical. According to previous experiments the drilling, probably with a hollow long bone with sand as the grinding medium, takes about twice the time consumed in the shaping and polishing of the axe (Fenton, 1984; Olausson, 1998). The hole tends to be only 2 or 2.5 cm in diameter, making the attachment point of the shaft vulnerable to breakage. Clearly the axe was not intended to be used for protracted hacking or chopping. On the other hand, as a symbol of extensive labor and of violence, and at least temporarily as a practical weapon, it is most effective.

Coinciding with what is often seen as the intrusion of the Corded Ware Culture, or perhaps beginning even earlier, the construction of giant's churches, possible foundations of palisade enclosures or timber framed buildings, indicates the first material account in the north of significant extra-subsistence labor activity in the procurement context. In Paper III I described the labor involved in the clearance of boulder fields in as much detail as is practical, but of the reason for such activity we have only conjectures. Still, even the related clearance activity is uncharacteristic of anything that had taken place previously, so it must represent a significant change in attitudes toward labor. This I see as an indication that the tumult and uncertainty, brought on by the arrival of an unfamiliar culture with its

differing worldview, brought on new circumstances in the procuring communities where strong leaders were allowed to emerge and set the agenda.

The position of such leaders may have been precarious at best, and probably, as in many ethnographic examples (see e.g. Clastres, 1989; Wengrow & Graeber, 2015), the followers after a time simply stopped following. During the Middle and Late Neolithic, despite the evidence of intensified communal labor, the archaeological record of the procurer zone does not really contain individualistic material expressions of status, indicating that such hierarchical power relations did not reach habitual cultural acceptance.

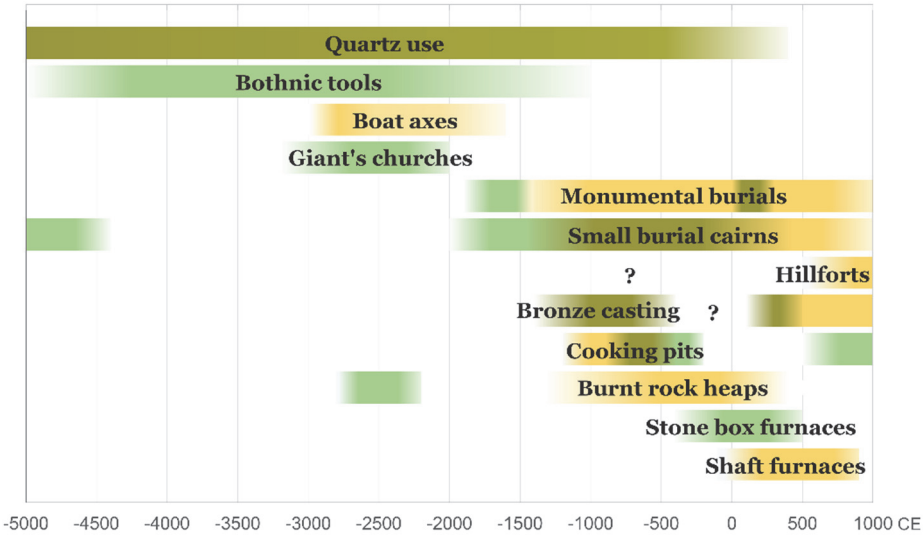

Fig. 21. Different labor activities analyzed in Paper III. Green indicates procurement context, yellow production, and olive-green activities occurring in both contexts. Originally published in Paper III.

In the Early Bronze Age, monumental burial structures emerged in the regions previously marked by the influence of the Corded Ware Culture. This, I argue, indicates that although material expressions changed through time, the ideological framework persisted. With the production ideological culture basing its livelihood on a dichotomy of humans/animals, which may have already resembled the later culture/nature dichotomy, hierarchical divisions within and, accordingly, between communities may have been an intrinsic logic from the onset. Monumental burials represent a hierarchy, not necessarily in the form of authoritarian leaders, but at the

very least of highly respected influential individuals. It is important to note the difference between the burial monuments of the producer and procurer zones. Nothing equaling the monuments of the south is found in the north, except for an apparently brief period in the Kemi-Tornio region between 100–400 CE, when several burial cairns reflect similar monumentalism.

A temporary ideological shift seems to have taken place in the Bothnian Arc in the Late Bronze Age. A change in the attitudes of coastal foragers toward labor may be signified by the increased use of cooking pits apparently between 800 and 200 BCE. Approximately a thousand cooking pits populate the Bothnian Arc, and it seems that the largest clusters, some containing up to a hundred pits, date to this period. But only a fragment of the whole, a dozen cooking pits, have been radiocarbon dated and the dating of the rest rely on shoreline displacement chronology, which may be incompatible with the logic of cooking pit use: there is no guarantee that they were even roughly coastal (see Paper II).

At the moment, the dating of large cooking pit clusters points to a relatively short cultural event, of some centuries, but this could very well change should a more systematic, and costly, dating regimen be applied. But, with the data we have at the moment, it would seem that surplus production could have been applied in the region during this period. This may relate to overexploitation, whether of fish, seals, or land mammals, or all of the above. Yet, it seems to have been followed by a short-lived diminishment of archaeological indicators of laborious activities, beginning with the cessation of the use of cooking pit clusters and ending with the establishment of monumental burial cairns mentioned earlier. This may yet again indicate the nullification of labor roles and hierarchy. If overexploitation was the reason why the cooking pit clusters emerged, the waning numbers of caught fauna may have accentuated the downside of the ideology of production, leading to the ceasing of surplus yielding practices. Or perhaps leaders merely lost their followers, yet again, signaling the fragility of local power relations.

In Paper III, I argue that the attitudes toward labor and hierarchy diverged during the prehistory of the region. Whereas the procurer communities repeatedly adopted labor roles which strengthened hierarchies only to abolish them some centuries later, hierarchies accumulated in the southern producer communities over the millennia, never truly dissolving. The dichotomy can therefore be seen as between preservation of anarchy and consolidation of hierarchy. Anarchy in this sense means the active maintenance of non-hierarchical decision-making, the objective of which is to ensure that the power to decide is not consigned to just a few entities (see Graeber, 2004). In an anarchistic context, cyclically abolishing

power relations before they are consolidated through habits and rituals into overall culture, makes political sense.

The political difference in the power relations in the two subsistence ideological contexts may have been due to a separation of worldviews. The worldview of foragers is often conceptualized as primarily imbedded within nature, as an irreducible part of it (see e.g. Bird-David, 1990; Porsanger, 2003; Kelly, 2013: 110–112). In contrast the agricultural worldview focuses on the control of plants and animals, and the separation of culture from nature. Agriculturalist communities rarely if ever reverted from the production ideology and hierarchical relations of their own free will. Because of this, development seems to have been cumulative over the millennia, with political structures occasionally being reorganized in the face of extreme circumstances. Thus, it should not surprise us that the early models by European archaeologists and historians produced evolutionary narratives of society and culture. They were part of an organization which did not habitually abolish itself, but instead focused on extending itself in time. Cyclical abolishment of production and hierarchy was either an alien concept to them, or an indicator of a failed civilization.

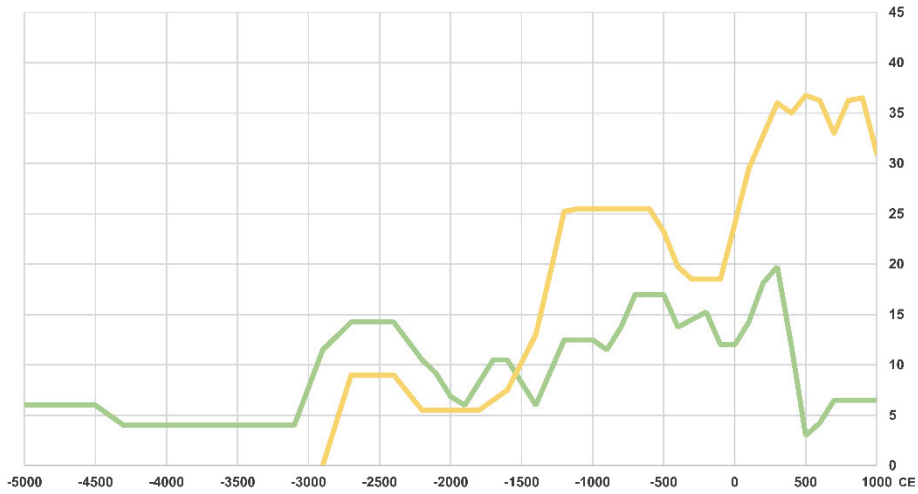

Fig. 22. Graph of labor activity averages in the two contexts: Green indicates the average of labor intensity and expertise within the procurer context, and yellow the producer context. The parameters are published in Paper III.

The production ideological heritage may have been among the reasons why Christianity was rapidly adopted throughout the southern regions of Fennoscandia within a few centuries in the early 2nd millennium CE (see e.g. Haggrén, 2015: 379–408), while only sluggishly spreading amongst the vast variety of northern communities, with Christian missionary work continuing among the Sámi at least until the 19th century (see Valkonen, 2014). In the south, early missionaries could convince local political actors who could argue for conversion in terms that resonated with the locals or resort to violence. In the north, conversion took place outside of an inherent hierarchical structure. It must have been a difficult task to argue for a single invisible and improbable god replacing all the empirical deities of rivers, promontories, animals, forests, and sky, without hierarchical coercion.

On the Bothnian Arc, the ideology of procurement was gradually displaced by the ideology of production, as indicated by the emergence of the Birkarl, a rather mystic historical group practicing taxation and trade in the region (see Bergman & Edlund, 2016; Kuusela et al., 2016). In the 17th century, after diminishing the influence of the Birkarls by establishing wealth-funneling towns along the coast, the state of Sweden began a concerted effort to convert the peoples of the north to Christianity (Valkonen, 2000). Eventually through political influence, colonialism, taxation, and violence, the procurer world became subservient to the ideology of production (see e.g. Massa, 1988: 26–29; Ingold, 1988: 126–130; Pennanen, 2003; Näkkäläjärvi, 2003; Broadbent, 2010: 207–209, 213–215; Hansen, 2012; Lehtola, 2015). Nevertheless, the modern Sámi culture still retains many elements of the procurer worldview (see e.g. Helander, 2000).

6.2 Death: multivalent responses to a universal equalizer (Paper IV)

In the abstract of Paper IV, 'Living with death', I and my coauthor Ville Hakamäki started off with a joke of a kind, when we said that 'mortuary practices […] are interpreted here *rather unconventionally* as expressions of morality.' This, originally without the emphasis, is admittedly not funny, but it may be provocative. I hope the readers are more provoked by the emphasis than what follows it, because that would indicate that burials as expressions of morality is a self-evident issue. Yet, I suspect it is not.

Before conducting the analysis for this paper, we first presented its theoretical framework in the 2017 Workshop of Finnish Archaeology of Religion in Tvärminne. We were met with skepticism. We argued that the underlying motive for mortuary practices in general is and has always been respect. In essence the treatment of the

cadaver—in mortuary, not in medical context—is an expression of respect by those performing the related actions. I have found that too often in archaeology burials are interpreted in terms of power relations and territorial politics, as I had also done in Paper I and partly in Paper III. While such interpretations have often been constructive, we wondered what insight we might gain from looking at mortuary practices from the perspective of what they all had in common. The issue had begun brewing in my head after the death and funeral of my grandmother in 2016. I simply could not recall reading any archaeological interpretation that resonated with the occasion, which indicated to me a disconnect between archaeological interpretation and reality.

The criticism we faced at Tvärminne varied from the argument that not all burials are performed out of respect, to the claim that the main goal of mortuary practices, instead of showing respect, is simply to get rid of the dead body. A question that was raised was why look for a shared motive in the first place. I was completely dumbfounded by this question, and ultimately I had no answer. Instead I sheepishly retreated, jokingly murmuring that 'I had some lofty idea, but I cannot for the life of me remember what I was thinking.'

After the workshop, on regaining my senses, I recognized that I had to indeed formulate an answer. So, as it is easier to put it in writing, I will provide it here. To us, it was imperative that we study the mortuary record in a timeless fashion, not focusing on the evolution of this or that practice. Instead we had to recognize that the objective in all practices is in a general sense the same. After all, everyone experiences death, of others and ultimately the self, so the issue is shared by all. Studying mortuary practices without a shared *interpretational key* (see Paper IV) would simply result in a descriptive study denoting the variety of burial practices.

My first impulse had been that the key was grief, and what connected the logic of burial was the expression of it. I had certainly experienced this myself, and grief has been abundantly expressed in practically all ethnographic examples I have come across. But there are exceptions. Christianity for one insists that their dead are in fact better off in a world of everlasting happiness and serenity, in contrast to the strife of earthbound existence. Thus, a true believer should be glad of a fellow's demise, although this is rarely so. Still, this indicates that grief in the mortuary context is not necessarily a shared attribute. This is echoed also by the expression of relief when the deceased has succumbed after lingering in prolonged pain, or jubilation when a hated enemy has been killed.

Contemplating further, I came to the conclusion that respect acts as an umbrella category for grief directed toward someone. It is difficult to fathom a situation

where the death of a person that you do not respect at all evokes grief. It is an instance where the person "had it coming," which must be a common expression in all the world's languages. If grief requisites respect, and respect is a frequently expressed motor in mortuary practices and rituals ("we have come here to honor…"), it represents a shared concept, suitable in this instance as an interpretational key.

Respect, as argued by philosopher Joel Feinberg (1973), is three-fold: *respekt* or dread, observance, and reverence. Dread is fearful wariness of its object, observance the acknowledgement of the object's intrinsic value, and reverence a feeling of awe toward the object. Respect, then, evokes a range of emotions, from fear to love, sometimes all at once.

Let us then consider a disrespectful grave. The best fitting antonym for all three of Feinberg's categories is contempt. So what we should look for in a disrespectful burial are expressions of contempt. Contempt manifesting itself in the act of killing is easier to observe than in burials. In hate crimes, especially racist or political murder, contempt is often a prerequisite. In Europe, politically manufactured contempt allowed the Holocaust, where millions of mainly Jewish victims were not only murdered but also their morally correct mortuary treatment was obstructed, either by incineration of bodies or mass burial. In the US, victims of lynching, if buried, were usually buried by their kin, not by their murderers, who often left their victims hanging as a monuments to terrorize the oppressed. Mass graves, in Rwanda, Cambodia, the former Yugoslavia, Syria, Iraq, and countless others, could be argued to represent graves without respect and instead contempt. Not all mass graves, on the other hand, are contemptuous. The motive behind some have been to fearfully hide the evidence of monstrous deeds instead of erasing the memory of the victims.

In archaeology, mortuary sites frequently contain burials that seem out of place in some way or another. In Fennoscandian prehistory, human remains are a sporadic find also in dwelling sites (Ahola, 2019: 60). The term 'deviant burial' has come to denote such mortuary practices deviating from the norm (e.g. Murphy, 2008). Such deviancy is often inferred either as disrespect or fear toward a deviant person. Whether a stone is placed atop the burial, or a bone is missing, or the body contorted, the interpretation comes naturally: this deceased was explicitly disrespected or feared. In fact, *the archaeology of fear* has become its own genre (see e.g. Tsaliki, 2008).

This interpretational key, to me, is inherently problematic. First, on what basis do we exclaim deviations from the norm? Second, what are the benefits compared

to the risks of such interpretations? In order to ascertain deviancy, we first need to define 'the norm.'

In our analysis, we first made an archival survey of all at least partly excavated mortuary sites in the study region, mainly in the watershed of the Bothnian Bay, and then went through all the field documentations or, if the documentation was not accessible, the published studies featuring the site. With this information at hand, we collated the site data along with recurring features into a dataset of 150 sites, dating from the Mesolithic to the Industrial Age. What became clear early on is that 'the norm' was out of our reach. Instead, prior to state control, mortuary practices varied widely even at roughly contemporaneous sites. Collating all the information of individual burials only highlighted the richness of differences. Only with Christianity becoming the norm, in the centuries after the Late Iron Age, can burial traditions be said to homogenize. But still, pluralism persisted. For example, in the mountain regions of Scandinavia, exposed above-ground burials, some of which may well have been excarnations, were practiced until the Industrial Age of the 19th century CE, possibly even into the early 20th century (see Manker, 1961).

On the second point, I fear that deviancy is not an objective notion in archaeology. Instead, it is a moral judgement projected onto the past. Who is judged is not only the deceased ("this person was an outcast"), but also the community ("the locals were obsessed with superstitious fears"), and through it the culture as a whole ("such irrational fears were habitual throughout prehistory"). Thus, as we base the notion of deviancy on observations of practices about whose ramifications to the practitioners themselves we have no objective knowledge of, we are actually coloring the past with our own moral convictions. I would argue that without dogmatic control over mortuary practices, asserted by a priesthood or legislative council, deviant burials are actually the norm, undermining the concept to the point of redundancy.

Criticizing the concept of deviant burials is nothing new. On the year I began studying archaeology, Edeltraud Aspöck (2008) questioned the usefulness of the concept. The year I graduated, Leszek Gardeła (2013) further expanded the critique.

> […] I would strongly agree with Aspöck that the term "deviant burial" should be seriously reconsidered. It is too biased to encompass the changing perceptions of the dying and the dead as well as manifold actions which may have occurred at the graveside. […] To my mind, seeking a different term for the peculiar (to our eyes) funerary behaviour is not a solution. Rather, we should try to embrace the multivalence of all sorts of actions and reactions to

> what may have happened at the graveside and try to approach their diverse archaeological remains with an open mind. (Gardeła, 2013)

So continuing with an open mind, we can still account for the arguably rare cases where disrespect may have been a guiding force in the conduction of a burial. We understood both respect and disrespect as expressions of morality, the determination of right and wrong. By studying mortuary practices as expressions of morality accounts for wider multivalence. Certainly in our data, there was nothing to suggest a burial conducted in a way that the practitioners themselves considered morally wrong.

We had been strongly influenced by ethnographies, but probably none more than an account of a Chukchi burial in Northeast Siberia. Admittedly, fear of the deceased is a recurring theme in the study, one which seems to have captivated the ethnographer, who mentions grief only passingly, seemingly with little interest. In any case, according to the account, the deceased was solemnly placed in a shallow open stone encirclement and left exposed for days. The attendants then returned to observe the state of decomposition and whether animals had mutilated the body. All signs of mutilation were taken as morally appropriate. The participants proceeded with a feast of their own, offering a share of their food to the deceased. Several of such visits would be undertaken, with no moral scruples about viewing a decomposing and gradually defleshing body of a loved one (see Bogoras, 1904: 526–536).

A third of the way around the world from this, taking place in the rainforests of the Amazon, a Bororo way of burial was described by Claude Lévi-Strauss, with the caveat that he himself did not witness the actual proceeding.

> First, the body is put in a ditch, covered with branches, in the middle of the village, and then, when putrefaction has been completed, the bones are washed in the river. Next, they are painted and ornamented with feather-mosaics stuck on with glue and, finally, they are sent down in a basket to the bottom of a lake or a running stream. (Lévi-Strauss, 1961: 219)

Furthermore, sky burials, where the cadaver is exposed to the elements by being placed on an elevated plain, either on a scaffold, as was common among the native people of North America, or as in Mazdaist Iran, on a specially built towers, are in clear contradiction to our current Western ideas of a moral burial. In the Western world, the sight of a dead body, especially decomposed or mutilated, is immoral in itself. To the Western eyes, unfamiliar mortuary traditions, such as the above, might

be interpreted as rather disrespectful, if not for the protesting words of their performers. And in archaeological research our respondents are almost always mute.

So, in the end, we decided to proceed with respect and morality as the key to interpreting our data. What we found with this key are connections between mortuary practices which otherwise might have gone undetected.

It is hardly surprising that the locations where mortuary sites had mainly been established were somehow geographically significant. Two main categories emerged: water connection and promontory connection. The water connection was found throughout prehistory, with burial sites established on coastal islands, islands on rivers and lakes, but also alongside rivers (see also Herva & Ylimaunu, 2014). Burials situated on islands have previously been interpreted as either the expression of fear of the undead, who, it is commonly said, cannot cross water, or as an attempt to inhibit animals from disturbing the remains. Folklorist Anna-Leena Siikala interpreted the first belief, somewhat circularly, as evidence of a widespread belief that the domain of the dead was situated 'across the water' (Siikala, 2014: 173). Regarding the second interpretation, my colleague Ville Hakamäki pointed out, rather ingeniously, that most animals can swim. Regarding the first, not all burials were connected to water, which undermines the universalism of Siikala's interpretation. Perhaps some of the dead were not feared, and thus they were not corralled with barriers of water, but then again, on what basis do we say the others were feared? Because they occupied the most visible locations?

The high visibility of burials is notable especially when they are placed on promontories. In archaeological surveys of the region, it is customary to search for burial cairns and stone settings on top of hills and smaller elevations, where they are most commonly found. Some of these may have been coastal islands in their time, but some were definitely not, especially in the southern parts of Central Fennoscandia and on the region east of the Kvarken, where on the other hand a possibly bloated number of unverified cairns have been registered (see a book review by Jari-Matti Kuusela in *Fennoscandia archaeologica* XXXI). Again, the placement of burials on hilltops could be interpreted as fear of the dead, should we choose to do so. Yet, there is no reason to argue so. In the long run, to alleviate mental stress, would you not think that traditions should have emerged by popular consent that relegated the feared dead by moral right to inaccessible places where all the region's dead where collated, so that accidental run-ins with them could be best avoided? Such a method was later adopted, as the dead were interred in church cemeteries, surrounded by walls keeping the dead inside.

While conducting the study, it became clear to me that the consolidation of Christianity and the state represents a significant break in time. This break is accentuated by the relatively abrupt change of the role of fire in mortuary practices. Fire was arguably ingrained in red ochre already at the beginning of our timeframe. At the Late Mesolithic site of Vepsänkangas [973010046] in Oulu, in a hearth excavated by archaeologist Satu Koivisto in 1998, a ceramic vessel holding red ochre had apparently shattered during heating (Koivisto, 1999). The heating of the pigment enriches the color (e.g. Zagorska, 2008), but in Paper IV we also argued, that in doing so red ochre incorporates the essence of fire within it. In Tainiaro [751010040], in what is apparently a large Late Mesolithic cemetery, red ochre was not a necessary feature in burials, and in many graves its place seems to have had been taken by burnt sand with a slighter red hue. Thus, we surmised, the strong red color may not have had been as important as how the matter was prepared through burning.

Cremation does not seem to have been a significant mortuary practice in the region until the Early Bronze Age. Yet, sporadic finds of cremated human bone in the Neolithic (see Ahola, 2019: 60) indicate that a form of fire burial, where the body was perhaps not completely incinerated, existed even before cremations were adopted in a manner that produced a permanent record, i.e. cremated bones. Only slightly burnt bones may not have survived the reductive processes of time and soil chemistry. Still, with the few bones recorded and with the link between red ochre and burnt sand, it seems ever clearer that fire had a role in mortuary practices very early on and continued to have a role throughout prehistory, until being banned by Christianity.

This ban again highlights the contrast between the Christian period and the preceding prehistory. Local deathways seem to have gone through a shock. Within a few centuries, previous traditions and habits regarding the moral way to confront death were abolished. Instead, external practices, dictated from the outside with the authority of religious dogma and state control, were established.

The urgency and uniformity of Christian burial practice suggested to us that the moral way to handle the dead became a much heavier issue than in previous times. Simply put, the stakes seem higher. Hell and purgatory had been adopted into the Christian dogma prior to its spread to the region. There is no evidence that such concepts, according to which the dead were cursed or set to linger in a liminal space, had necessarily existed previously, as we will discuss shortly. Understandably though, such evidence would be hard to come by through

archaeological means. Yet, interpretations of fear of the dead, by projecting folkloristic sources onto prehistory, does not occupy any firmer ground.

The prevalence of fear of the dead in the region's historical times has been noted in many instances (e.g. Schefferus, 1674/1963: 410–418; Waronen, 1895; Harva, 1948/2019: 488–511; Siikala, 1992). Folklores and ethnographies extolling such fears were recorded from the 17th century CE onwards. Archaeologist Milton Núñez, for one, noted this historical 'dread of the dead' and projected it all the way to prehistoric times (Núñez, 2015: 97–99). The argument goes that the concept of contagious death, *kalma*, predates Christianity in the region.

Then again, this interpretation remains problematic. There is no evidence to suggest that contagious diseases were a serious danger in the region prior to the devastation caused by epidemics from the 15th century onwards. Although there is no evidence that the bubonic plague of mid-14th century spread to Finland or Northern Sweden (see e.g. Vahtola, 2003/2017: 47–48), places which were mostly devoid of towns and other major population centers in that period, the stories that emerged from the horror undoubtedly spread. Could *kalma* have gained its significance and amplified the fear of the dead only after the emergence of a clear and present reason for it? Similarly we might ask, how the concept of *kalma* could have gone untransformed with the realization of the mortal danger posed by dead bodies in the event of epidemic diseases? Also, a tentative link between the fear of the undead and Christian dogma, with its talk of resurrection, purgatory, and hellfire, has not gone unnoticed (see e.g. Moilanen, 2017: 138).

To me, it seems only logical that the prehistoric inhabitants of Fennoscandia were more at ease with death than people taking their cues from the current Nordic culture. We have outsourced mortuary practices, and for the most part, do not partake in them, except in the form of a ready-made ceremony (e.g. Pajari, 2014). We obliterate our deceased in crematoriums, where even the last bone is ground to dust, or we hide our dead in closed caskets. We fear dead bodies, not only because of the diseases we are concerned we might catch, but also since they are an immoral sight. We might do ourselves a favor if we retuned ourselves to death and the dead body, which would perhaps alleviate some of the anxiety our current society dwells on (see e.g. Rinpoche, 1997/2004; Weiner, 2015).

I argue that even though they differed in issues of labor and hierarchy, the prehistoric communities of both the northern procurer and southern producer zones shared mostly similar cosmologies related to death. This interpretation is skewed by the vast contrast perceived between prehistoric and Christian traditions, which blinds us from seeing what might be called "normative differences" within the

prehistoric mortuary record. It should be noted that the methodology of this study, and its long-term perspective, undoubtedly places too much emphasis on the perceived contrast of the prehistoric and historical periods, maintaining an unnecessary borderline between the two. In a recent study of Late Iron Age burial practices, archaeologist Hanna-Leena Puolakka argues that contemporary burials are not strictly Christian nor pre-Christian, but each burial should be examined as its own context (Puolakka, 2019). Hopefully, with such a nuanced approach, in line with the ongoing re-examination of contemporary local events (e.g. Kuusela et al, 2020; Nurmi et al., 2020), the accentuated break between prehistory and history may be replaced with a clearer understanding. Still, for the time being, I would argue that during prehistory there might not have been much tension between different mortuary practices, but that multivalence, as argued by Leszek Gardeła (2013), was prevalent in the handling of the dead in prehistoric Central Fennoscandia. At some point in time during the 2nd millennium CE, this multivalence had become an immorality, as conformity became desirable. As argued in Paper IV, current cremation practices are not relatable to the prior cremations present in the archaeological record, since they are hidden *obliterations*, forgoing the spectacle for the senses that prehistoric cremations represent.

7 Analysis: Local identities of prehistoric communities

As the previous chapter focused on long-term cosmological issues on the larger geographical scale, this chapter makes a foray into the local level. We will note the different ways local traditions manifest archaeologically in a comparison between the regions surrounding two river estuaries on the Bothnian Arc. The analysis for Paper V originally encompassed a wider temporal range, but since the scale of academic papers does not favor verbosity, I scrapped half the analysis in writing the article. In the next section, I present the full analysis, with emphasis on the previously unpublished half. And finally, noting the variety of indicators of local identity, we will return to the very beginning, after a long and arduous narrative by evaluating whether the analyses and interpretations in Paper I, finalized in 2014, continue to hold water.

7.1 Comparative study of material identities of two river estuaries (Paper V with extended analysis)

The final paper, Paper V, represents my original research plan to a great extent as I envisioned it, except with a narrower regional focus. The original plan was to systematically compare the long-term archaeological record of different river estuaries and assess the variation in the regional prehistories, formulating a grounded interpretation of what the local prehistoric society truly was. But when I chose to reorganize the dissertation from a monograph into a series of articles, that plan could no longer be realized. Call it inexperience or realism, but I simply could not see how I would rationalize splicing the data and analyses in separate articles. The format would inherently create arbitrary borderlines in time and space. In hindsight, it could also be that I simply lacked the necessary discipline.

Compared with the original extent of the analysis, Paper V offers only a glimpse into the identities of local communities. But even this mere glimpse gives us enough information to assess the impact of repopulating the prehistoric world with local communities, replacing monolithic societies of various and often undefined scales. The two compared regions surround the river estuaries of the Oulujoki River and the Kemijoki River (Fig. 23). The regions were selected based on local geography, within 10 km of the contemporary seashore, and within 40 km of coastal distance from the central river estuary. By considering such regions in terms of living geography (see Chapter 4.2), a magnetic relation between local

communities and river valleys can be established. The analysis had to be set mainly on the coast, since sites which cannot be established as coastal may occupy many temporalities after their *terminus post quem*, or emergence from the sea. The maximum coastal range applied here roughly represents the distance of a day's journey from the central estuary to the edge by boat (8 hours, 5 km/h; see Rowley-Conwy & Piper, 2017: Table 1). It is probable that the two defined regions actually contained several contemporaneous communities with their unique identities. But performing the analysis with narrower geography would leave more gaps in the material record. It is advisable, in future studies, to strive for more focused analyses by focusing less on the long-term.

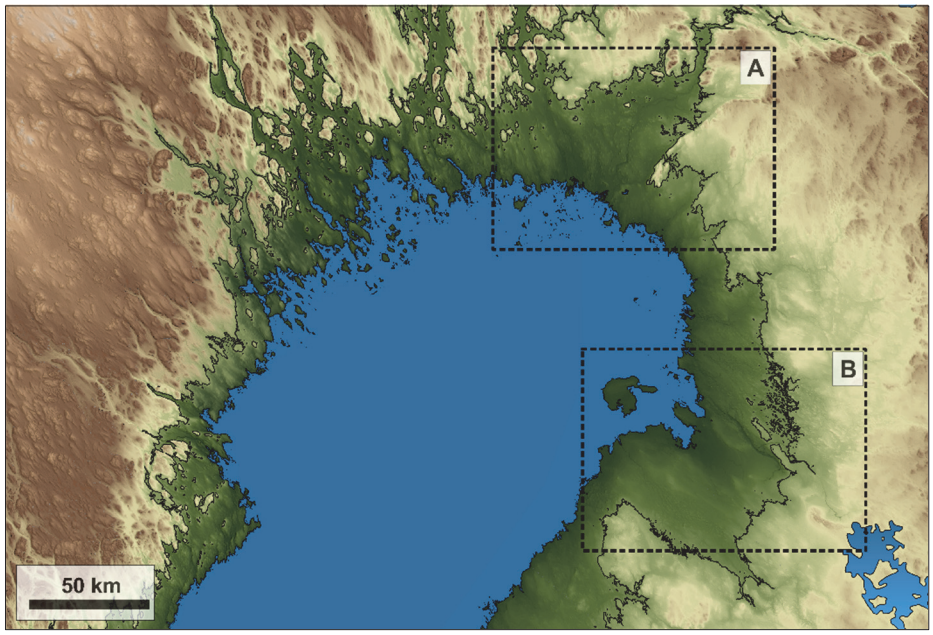

Fig. 23. The two focus regions under comparison. A: The region surrounding the Kemijoki River estuary; B: The region surrounding the Oulujoki River estuary. The outer contouring indicates the shoreline of 4800 BCE, while the inner contouring is the present-day coastline. DEM by NLSF and Landmäteriet.

Late Mesolithic and local stone use

Around 4800 BCE, the two regions surrounding the river estuaries of Oulujoki River and Kemijoki River formed distinct bay areas on the eastern coast of the Bothnian Arc, separated by less than 100 km. The Oulujoki River estuary was surrounded by a slightly concave shoreline, with a 25 km long sandy ridge formation extending northwest to the sea, which sheltered an archipelago on the north side. Meanwhile, the Kemijoki River estuary was a 30 km wide bay, leading to a long winding fjord extending far inland. Just south of the wider bay, a smaller 12 km wide bay contains the peculiar archaeological site of Tainiaro (see Fig. 24: #6).

Tainiaro, as mentioned several times already, is an apparent large inhumation cemetery with more than 40 burials that dates approximately to 4800–4400 BCE (see Hakonen, 2019a; 2019b; also Paper IV). At face value, it is unique in the region, as no other such cemeteries have been discovered prior to the Late Iron Age. Interpreting the site as contemporaneously unique is problematic since the site was originally discovered due to the lucky placement of a sand extraction pit, aided by the awareness of locals in identifying the Stone Age artefacts they came across with. We cannot truly say whether similar sites existed in neighboring regions or not. Perhaps they simply remain to be discovered.

But the other unusual characteristic of the site is the use of local stone. Approximately 80% of stone refuse found at the site is what archaeologist Hannu Kotivuori called mafic tuffite, likely originating from sources in the region of the lower Kemijoki River (see e.g. Kotivuori, 1996a: 39; Kelloniemi, 2021). The usual mainstay of local prehistoric tool making, quartz, is in the minority. Along the Kemijoki River Valley and its immediate surroundings, mafic tuffite maintained its popularity in the few contemporary sites that have been excavated, such as Veskankangas and Jokkavaara (see Fig. 24: #3 and #7). In both places around 30% of the excavated debitage consists of mafic tuffite. But in the Oulujoki River Valley, at the partly excavated sites of Latokangas and Vepsänkangas (Fig. 24: #10 and #11), mafic tuffite refuse, which indicates local processing of stone, represents only 1/10th or less of all discards. This indicates that the stone was worked primarily in the northern region, where it can be easily obtained, while the southern region relied mainly on quartz (Paper V).

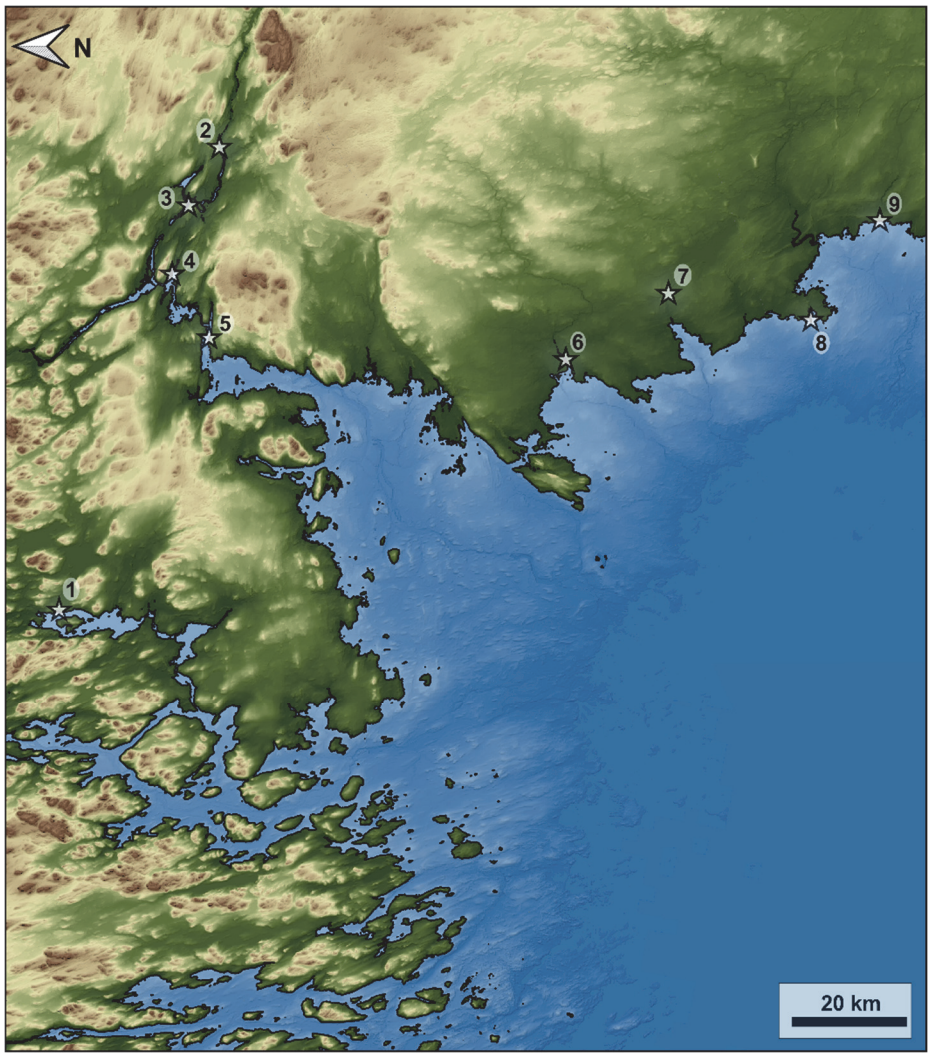

Fig. 24. The Late Mesolithic (4800 BCE) coastline surrounding the Kemijoki River estuary. The numbered sites are 1. Kaaranes 1-2 [854010024]; 2. Ollonen [699010061]; 3. Jokkavaara [699010340]; 4. Kolpene [699010252]; 5. Hietavaara [699010140]; 6. Tainiaro [751010040]; 7. Veskankangas [1000000035]; 8. Keelaharju [139010014]; 9. Pahkakoski 1 [972010014]. Note the direction of the north arrow. DEM by NLSF and Landmäteriet.

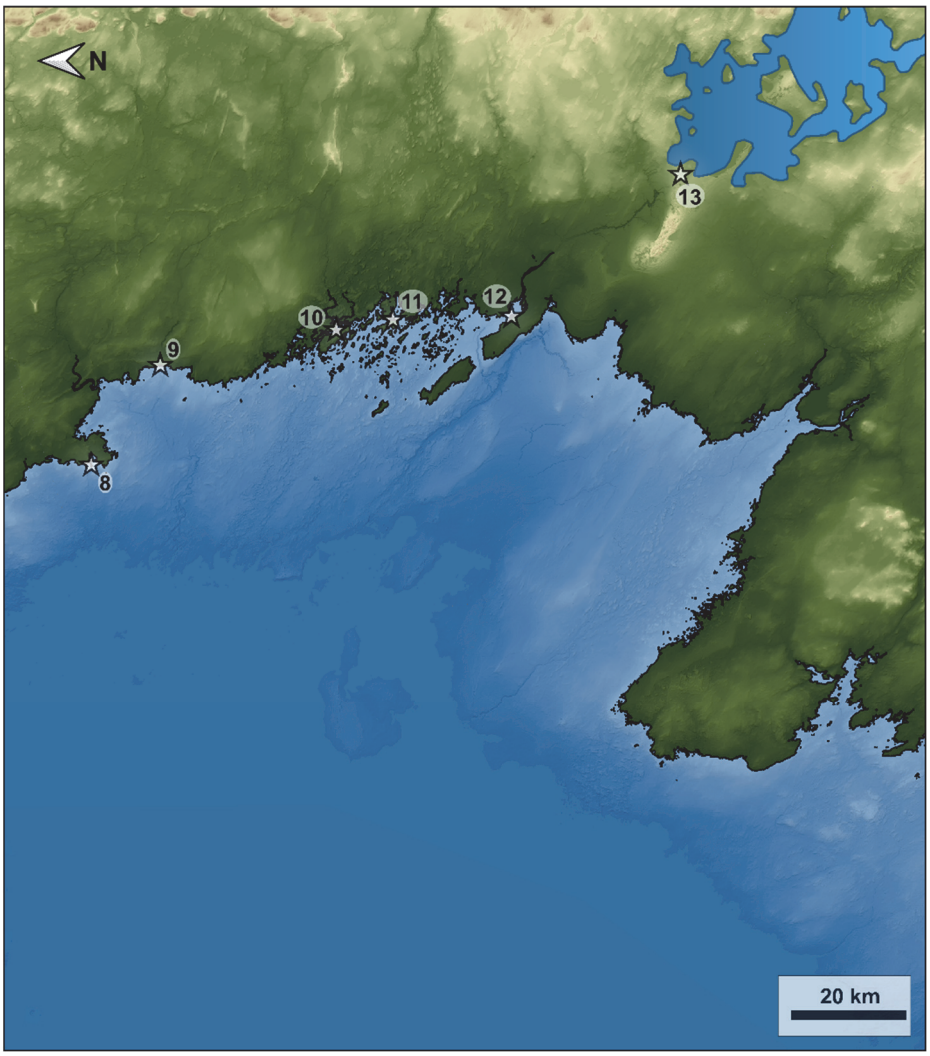

Fig. 25. The Late Mesolithic (4800 BCE) coastline surrounding the Oulujoki River estuary. The numbered sites are 8. Keelaharju [139010014]; 9. Pahkakoski 1 [972010014]; 10. Latokangas [973010028]; 11. Vepsänkangas [973010046]; 12. Pyhänniska [889010078]; 13. Nimisjärvi Järventaka [785010011]. DEM by NLSF.

Now, it is not surprising that the material was used more frequently where it was abundant. What is surprising is that according to the artefact assemblages of Tainiaro, the material was used for some time as an actual replacement for quartz. So-called scrapers are common prehistoric tools, made mostly of quartz. Making one is relatively easy. Archaeologist and university lecturer Jari Okkonen, in our many discussions in the archaeology department's former GIS-laboratory described vividly how quartz could have been used as prehistoric Swiss army knife. Instead of being a poor person's flint substitute, which requires an infuriating measure of practice and skill to be worked into coherent tools, a quartz nodule could actually produce a number of blades useful in different circumstances simply by being broken into random flakes (see also Driscoll, 2011). Choosing the most suitable flake, the user could then refine it into a workable tool best suited for the occasion. For example, by serrating a larger flake you would have a scraper. So, according to Okkonen, just carrying along a quartz core nodule would have made you ready for anything. Since flint was brought from far away, and apparently in relatively small quantities, locally procurable quartz offered the freedom for more wasteful use.

Contrasting with quartz, mafic tuffite tends to break lengthwise, making it rather predictable as a material. Later on it was used almost solely for the production of Bothnic tools, as outlined in Chapter 5.1. But during 5500–4000 BCE, it seems to have been used for a multitude of purposes, blades and scrapers included. A special tool type emerged in the coastal region of the Kemijoki River, where 'hafted scrapers' (see Paper V: Fig. 4) of mafic tuffite can be found, although apparently only from the Late Mesolithic period (5200–4000 BCE). It seems the locals were experimenting with this new-found material, producing short-lived object types whose use does not seem to have extended to the Neolithic.

The effort that was invested in the search for a purely local stone, apparently either to supplement quartz use or to replace it altogether, seems to me highly impractical. There is no indication that there was a shortage of quartz in the Kemijoki River valley, and in fact several possible quartz quarries that were available throughout the timeframe have been discovered in the region. Thus, I suspect that the experimentation with unique local stone was not a matter of practical adaptation (cf. Jørgensen, 2020), but of identity. By adopting a novel local material for stone tool making and ultimately conforming the new toolset to suit the whims of the material, the local communities gained the knowledge to produce customized tools, for the purposes of elevating their communal identity.

Admittedly, this is only one scenario. There could have been many other reasons. But this is definitely how such a differentiation of simple tools seems to

play out from the perspective of material culture. We already interpreted a similar symbolic representation of identity regarding the Corded Ware Culture's boat axes. I would argue that a similar interpretative key should apply to both cases. But it is clear in any case that the material identity of the two compared regions diverged already in the Late Mesolithic.

The Neolithic and the divergence of dwelling habits

By the beginning of the Early Neolithic (4000–3200 BCE), the experiments with mafic tuffite had by-and-large ceased, with only the elongated Bothnic tools having stayed in use. The fjord of the Kemijoki River Valley was beginning to close, with new lowlands rising to surround the estuary. In the region of the Oulujoki River, emerging land was fusing the former splintered archipelago into a bay filled with larger slowly expanding islands. The former seabed on the mainland lay amid hills dotting the land north of the prominent ridge formation. When land uplift blocked the route of the Oulujoki River from reaching the sea, the river did not bother circumventing the high 25 km long sand ridge but forcefully punched through it, forming terraced river shores on the new estuary at the center of the ridge. In the same region further to the southwest on the estuary of the Siikajoki River, lowlands were rising rapidly, forming a new yet temporary archipelago. The first Neolithic villages may have been established on these islands and on the estuary of the Oulujoki River, where the split ridge stood stoically.

In Paper V, I estimated the period when most of the Neolithic villages were established in both regions as 3500–3000 BCE. According to shoreline displacement chronology, the earliest possible establishment date for two villages located at high elevations is 3900 BCE, both of which, Rekikylä [973010051] and Marjokangas 1 [1000027334], are in the southern region. A site located on the Oulujoki River estuary, Pyhäkoski 1 [494010073], could have also been established several centuries earlier, but it has maintained a stable riverine location to this day, so it was as likely to have been established at any later date. The village tradition seems to have emerged in the Kemijoki River estuary after 3600 BCE, apparently centuries after the first villages of the Oulujoki River region.

The state of research of such Neolithic villages has progressed recently with gusto, with lidar surveys revealing new sites every year. Archaeologist Petro Pesonen has recently identified an intriguing dwelling form, the ring-formed village (see Tallavaara & Pesonen, 2018). Pesonen first documented two adjacent ring-formed village sites, Turusensaari [1000027397] and Kivimaankangas 1

[1000027396] (Pesonen, 2017). These two roughly rectangular formations of 14 to 17 dwelling depressions suggest contemporaneity, and in that case their layout may have had a defensive function. Previously, archaeologist Pentti Koivunen interpreted the site of Kierikkisaari [972010009], 100 km north-northeast, as a rectangular pile-settlement with an inner courtyard, explaining the dispersal of flint arrow heads at the site as an imprint of an attack (see Koivunen, 2002). The illustration of the pile-settlement envisioned by Koivunen certainly fits well with the layout of the two ring-formed villages documented by Pesonen (compare Koivunen, 2002: Fig. 2 with Pesonen, 2017: 7–8).

At the moment, less than ten possible ring-formed village sites can be identified in lidar data—depending on the interpreter—with seven of them south of the Oulujoki River estuary, near the Siikajoki River. According to the shoreline chronology, the two documented sites, Turusensaari [1000027397] and Kivimaankangas 1 [1000027396], may date already from 4050 BCE, but they could also be later villages established on a lakeshore. The *terminus post quem* for the rest of these sites is after 3600 BCE. It would be an intriguing discovery if the tradition of Neolithic villages first began in the form of defensive enclosures, but at the moment, such an interpretation has to wait for either verification or dismissal based on excavated evidence.

With the onset of the Middle Neolithic (3200–2300 BCE), the material records of the two compared regions diverge further (see Fig. 26 and 27). A dozen giant's churches have been discovered on the bay surrounding the Oulujoki River estuary. Only three additional structures are found further north, the northernmost being Metelinkirkko, Huhinmaa [972010002] on the estuary of the Iijoki River, halfway between the two studied regions. The distribution of row-houses is similar, with more than a dozen dwellings with two or more adjacent rooms found in the southern region. Only two double-room dwellings have been located in the northern region. Again, the main distribution of such dwellings, especially those with more than two adjacent rooms, seems to halt somewhere in the region of the Iijoki River.

According to the shoreline displacement chronology, the use of large clusters of dwelling depressions ended at different times in the two regions. In the south, all the sites with over 20 depressions were apparently established prior to 3200 BCE, while in the north the last two of such clusters, Leilisuo koillinen [1000031123] and Kankaanjänkä [845010098], were established in between the shoreline phases of 3000 and 2700 BCE. A single radiocarbon date from Kankaanjänkä even dates a few centuries later, but it being only a single charcoal date (Kotivuori, 1998: 16), its veracity has not been established. If we look at the sites with clusters of more

than eight depressions, instead of 20 or more, the chronology moves forward, but maintains the same pattern: in the north, the last sites were established after 2400 BCE; in the south, after 2800 BCE.

Now, deciding what an analytically significant number of dwellings is per site is rather abstract, but my aim is not to define a village as a certain number of dwellings, but rather to show a pattern. This pattern, and the overall *tilt* in the relative chronologies of the two regions, could be explained by erroneous land uplift calculations, where instead of slightly faster uplift in the northern region, it actually experienced significantly slower uplift than the southern region. Yet, our current knowledge of local relative uplift points exactly the other way (see e.g. Poutanen & Steffen, 2016).

What seems more likely is that the communities of the two regions experienced similar material cultural shifts, but the events in the south preceded those in the north by a few centuries. The traditions of the two regions diverged more apparently during the Middle Neolithic, perhaps accentuated by the southern region's closer proximity to the regions further south, a context influenced by the archaeological Corded Ware Culture. On the other hand, as ring-formed villages, arranged with a seeming defensive posture, predate at least the majority of the giant's churches, and the southern CWC, the development of defensive architecture may have first occurred as a response to local events, instead of outside influence.

This is where I concluded Paper V. As I said before, this decision was due to the limits of the article format. But, with so much data left over from the laborious data gathering, consisting of a total of 1,245 sites, we can continue with the comparison until the "Mid-Iron Age forager gap." We will look at four further periods where the archaeological records of both the Oulujoki River region and the Kemijoki River region contain roughly contemporaneous material.

Fig. 26. The dwelling record of the Neolithic overlaid on the region surrounding the Kemijoki River estuary of 2700 BCE. Villages consist of 3–25 dwelling depressions while large villages contain up to 230. Over 10 m long depressions are categorized as large dwellings. Contouring set to 3900 BCE (high) and 2000 BCE (low). DEM by NLSF and Landmäteriet.

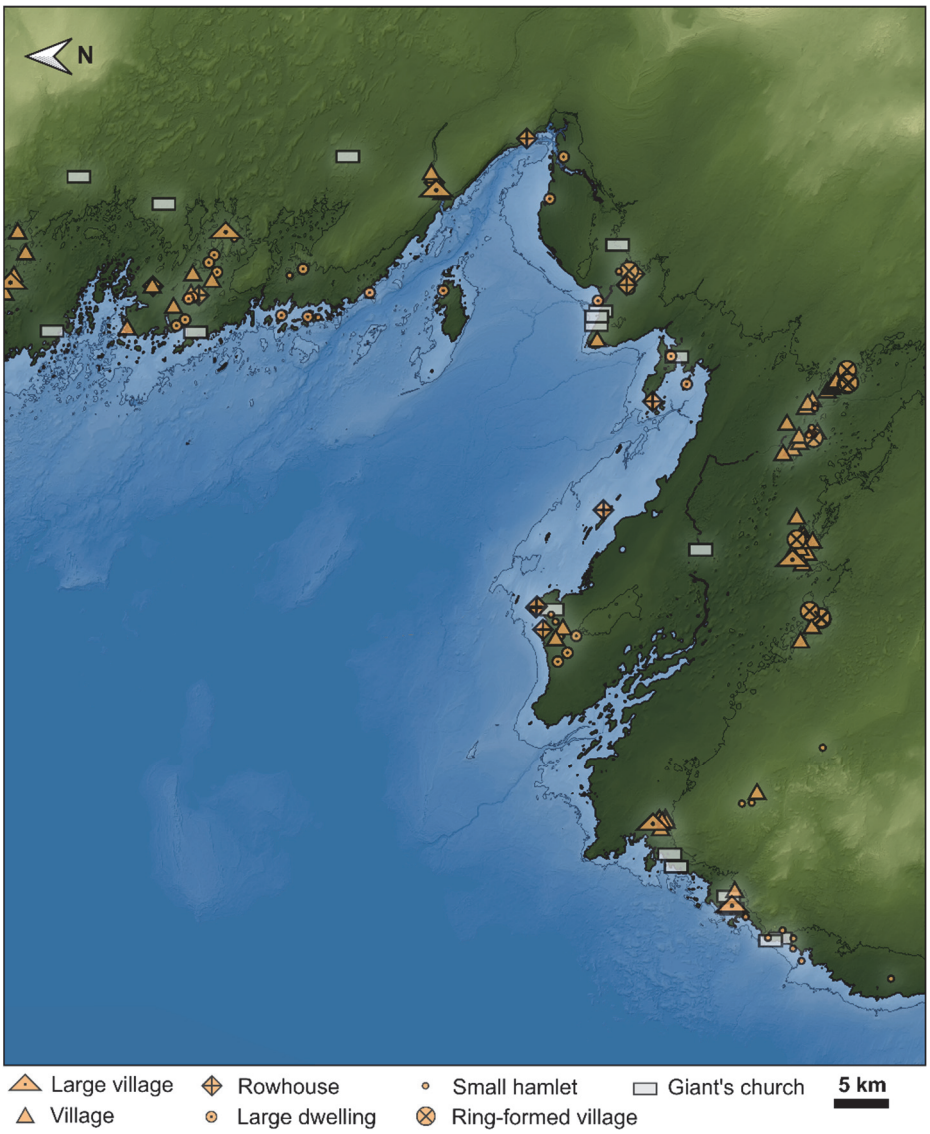

Fig. 27. The dwelling record of the Neolithic overlaid on the region surrounding the Oulujoki River estuary of 2700 BCE. Villages consist of 3–25 dwelling depressions while large villages contain up to 140. Over 10 m long depressions are categorized as large dwellings. Contouring set to 3900 BCE (high) and 2000 BCE (low). DEM by NLSF.

Early and Middle Bronze Age interlude

The Oulujoki River valley contains compelling evidence of Early and Middle Bronze Age activities. Unfortunately, the Kemijoki River region lacks a verified contemporary record, so we cannot perform a comprehensive comparison during the two periods. In the northern region, the locales of 60 archaeological sites emerged from the sea between 1900–1000 BCE, with their *terminus post quem* occurring in the Early and Middle Bronze Ages. These sites consist of approximately 200 cooking or trapping pits, 19 possible dwelling depressions, and over 20 cairns or stone settings. From these sites only four radiocarbon dates are available; three from cooking pits (see Table 5, first three sites) and one from a smaller hearth (2220±110 BP, Hel-3234), with all samples dating between 900 BCE and 250 CE. Taking into account the possibility of the aging deadwood effect (see e.g. Paper II; Bergman & Ramqvist, 2018) pushes the dates a century or so forward, so that the date range resides mostly in the Late Bronze Age and Early Iron Age.

The Oulujoki River region contains a numerally comparable set of 78 sites with the same *terminus post quem*. Included are nearly 500 earthen pits, which could mostly be either cooking or trapping pits, six dwelling depressions, and 17 cairns or stone settings. But the two datasets are made practically incompatible by the relatively extensive excavations of Halosentörmä [494010040] (e.g. Ikäheimo, 2005; 2015), Hangaskangas [564010051] (see Forss & Tuovinen, 2001; Ikäheimo, 2005; Ikäheimo, 2019), Hangaskangas E [1000006785] (Pesonen, 2013; Mikkola, 2015), and Peurasuo [564010048] (Ojanlatva & Alakärppä, 2002). The four sites are all situated in the contemporary Oulujoki River estuary, within a 1.7 km radius (see Fig. 28). Radiocarbon date records indicate that all these sites were active during the Early and Middle Bronze Age. Interestingly, artefact assemblages contain apparent mafic tuffite, both as objects and as debitage (see Ikäheimo, 2005; Pesonen, 2013; Rantanen, 2014). Whether its source is the Kemijoki River valley or not remains to be seen.

What the two relative records suggest is that in both places semi-subterranean dwelling tradition continued, with large up to 20 m long dwellings used in the south (see Ojanlatva & Alakärppä, 2002). In the north smaller unverified ones may have been used (see e.g. Ylimaunu, 1996: 210), but in much smaller numbers than previously. When adjusted in relation to the duration of the compared periods, the number of Early to Late Bronze Age dwelling depressions is only 1/50 compared to the Neolithic. It has been suggested that the human population plummeted accordingly, but the reasons for such decimation have not been explained (see

Chapter 9.4). It seems more likely that the semi-subterranean dwellings were mostly replaced by structures above ground-level, archaeological evidence of which is difficult to perceive.

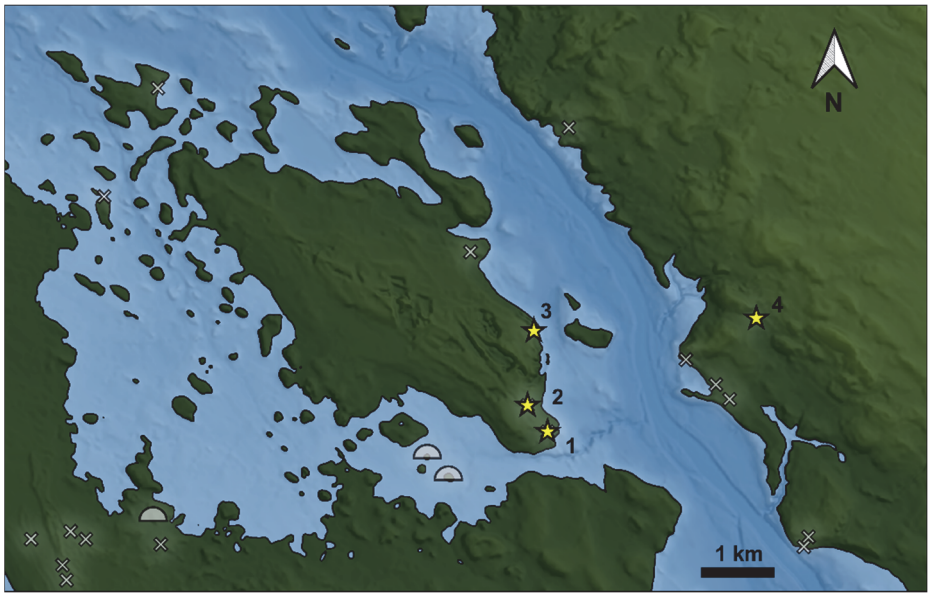

Fig. 28. The Hangaskangas Island and its environs, exemplifying a distinct local community. Stars indicate the sites of 1. Halosentörmä; 2. Hangaskangas; 3. Hangaskangas E; 4. Peurasuo, with a total of 13 excavations conducted at the four locations (see Fig. 29 for other symbols). DEM by NLSF.

Fig. 29. The dataset of the Early and Middle Bronze Age (shoreline set to 1200 BCE, higher contour at 2700 BCE) in the region surrounding the Kemijoki River estuary. DEM by NLSF and Landmäteriet.

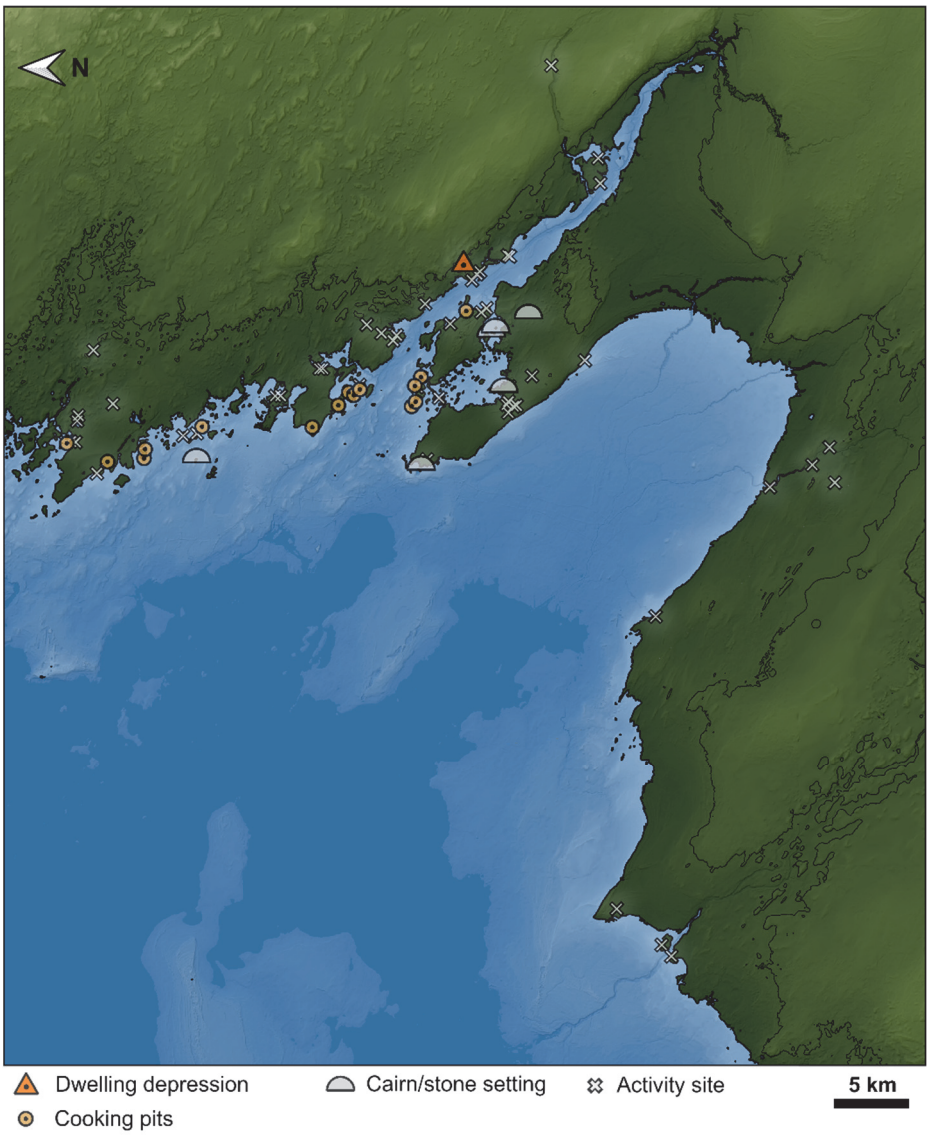

Fig. 30. The dataset of the Early and Middle Bronze Age (shoreline set to 1200 BCE, higher contour at 2700 BCE) in the region surrounding the Oulujoki River estuary. DEM by NLSF.

Cooking pit clusters as signs of Late Bronze Age cultural convergence

The Neolithic Bay of Tervola surrounding the contemporary Kemijoki River estuary closed up during the Early and Middle Bronze Ages. In 800 BCE, the river flowed through the newly emerged lowlands until reaching the sea, opening into a complex archipelago with new land continuing to rise. Land uplift formed islands, skerries, and ridges, on top of which several Bronze Age remnants reside today. The estuary of the Oulujoki River became likewise more constrained, with the island of Hangaskangas (Fig. 28) barricading the river and directing its flow northwest. A relatively small estuary, 5 km wide, was formed, with a coastal island chain extending from the southern shore of the estuary to the north. The Bay of Liminka, which had formed on the north side of a ridge formation blocking the Siikajoki River from flowing into the bay, had formed into a distinct U-shape. From there the coastline looped around to the northwest and continued further south.

At this stage, the main archaeological indicators are cooking pits, especially the use of them in large clusters. The largest clusters in the Kemijoki River estuary contain up to 40 pits, although without extensive probing or excavations these are often difficult to distinguish from trapping pits. Nevertheless, over 270 cooking pits have been identified in the region, and at least seven sites contain more than 10 cooking pits.

The analysis of radiocarbon dates in Paper II indicated that we have no certainty as to whether the majority of the cooking pits were dug close to the contemporary shore, making shoreline displacement chronology a highly questionable dating tool in the context. The prominent topography of the lower Kemijoki River adds even more uncertainty, since the high ridges occupied by cooking pits maintained their relative position to the shore for hundreds of years. While many cooking pit sites began emerging from the sea centuries before the Middle Bronze Age, four radiocarbon dates collected from the Kemijoki River region's cooking pits all point to later centuries, ranging from 900 BCE to 500 CE (see Table 5). However, the insufficiency of the limited radiocarbon date record hampers our understanding of this chronology.

Despite the uncertainties in the chronology of cooking pit sites, we may still make tentative interpretations, while keeping in mind that even small additions to the current data may overturn them. Focusing only on the seven sites of the northern region with more than 10 cooking pits reveals that all of them had properly emerged and were well over 300 m from the shore by 500 BCE. Adding to that the only radiocarbon date from a large cluster, specifically Rovavaara 6, suggests that these

clusters belong to the period prior to 400 BCE. At least some of the single pits were used until much later. On the Northwest Bothnian Arc, in Sweden, multiple cooking pits have been dated to ca. 600–1200 CE (Bergman & Ramqvist, 2018). There is yet no indication that such late use of cooking pits took place in the estuaries of the Kemijoki River, although the cooking pit at Kortejärvenkangas has been dated to 300–600 CE.

Similarly, there is yet no evidence of 1st millennium CE use of cooking pits in the Oulujoki River region. All the cooking pit sites, some with only a single pit, had emerged by 300 BCE. On the other hand, all sites with more than 4 pits had emerged already five centuries previously. The four available radiocarbon dates point to 800–200 BCE. All in all, because the issues with dating such sites—their apparent distance from the coast, and the lack of excavations and radiocarbon dating—we can only discuss the big picture (see also Kuusela, 2014a). We do not know how many cooking pits were used simultaneously or when they were used. We can only surmise that on the basis of the apparently similar high-volume use of cooking pits, the communities of the two regions seem to have become less divergent in their actions.

We have previously seen that this era, roughly the local Late Bronze Age (700–300 BCE), seems to have been an era of extensive contact networks throughout Eurasia. Similar bronze celt technology was in use throughout the Baltic and Northeast Atlantic coasts, and the East European Plain (see Chapter 5.2). Different elements required for bronze working spread from the Mediterranean to at least Southern Scandinavia, but possibly to the Bothnian Arc as well. Already in the Early Bronze Age (1700–1200 BCE), the community of the Hangaskangas Island had access to bronze seemingly from two different origins: from the east along the Kama River and from Southern Scandinavia (Ikäheimo, 2019), the latter which, based on local provenance studies, may have originated from the Mediterranean (Ling et al., 2014).

Such long distances covered by material networks may be reflected in the local records. Perhaps strict divergences between neighboring regions were not deemed as important as during the Middle and Late Neolithic. The networks seem to indicate an actual opening of the world. The term Axial Age was coined by Karl Jaspers for this purpose (see e.g. Graeber, 2011: 223–224; Scott, 2016: 20). Some individuals and groups may have undertaken long journeys to faraway places, either, as some have hypothesized, for the purposes of trade, or simply to explore and share gifts in the hopes that they may be reciprocated by either materially or tactfully with kindness. Kindness could manifest in elaborate feasts, the sharing of

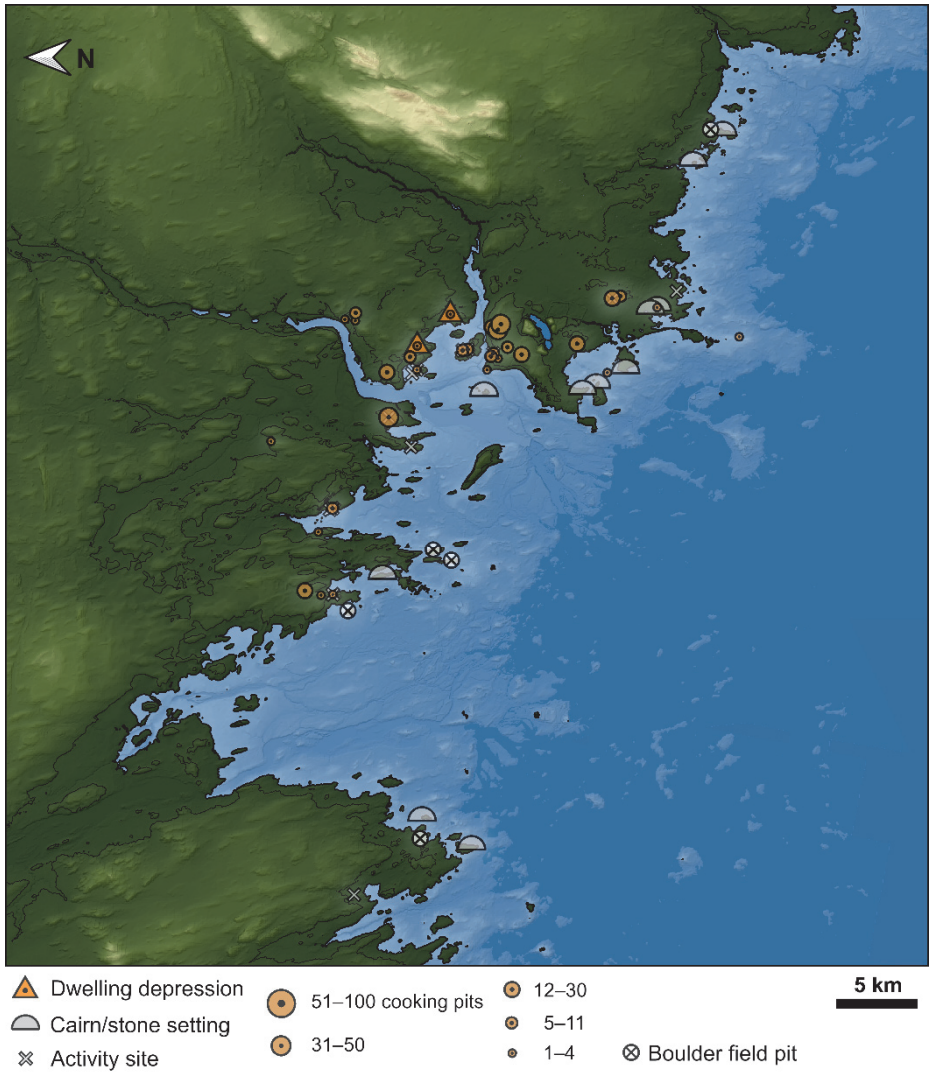

Fig. 31. Cooking pits and other mostly undated remnant sites of the region surrounding the Late Bronze Age Kemijoki River estuary. Shoreline set to 300 BCE, with the higher contour set to 1200 BCE. DEM by NLSF and Landmäteriet.

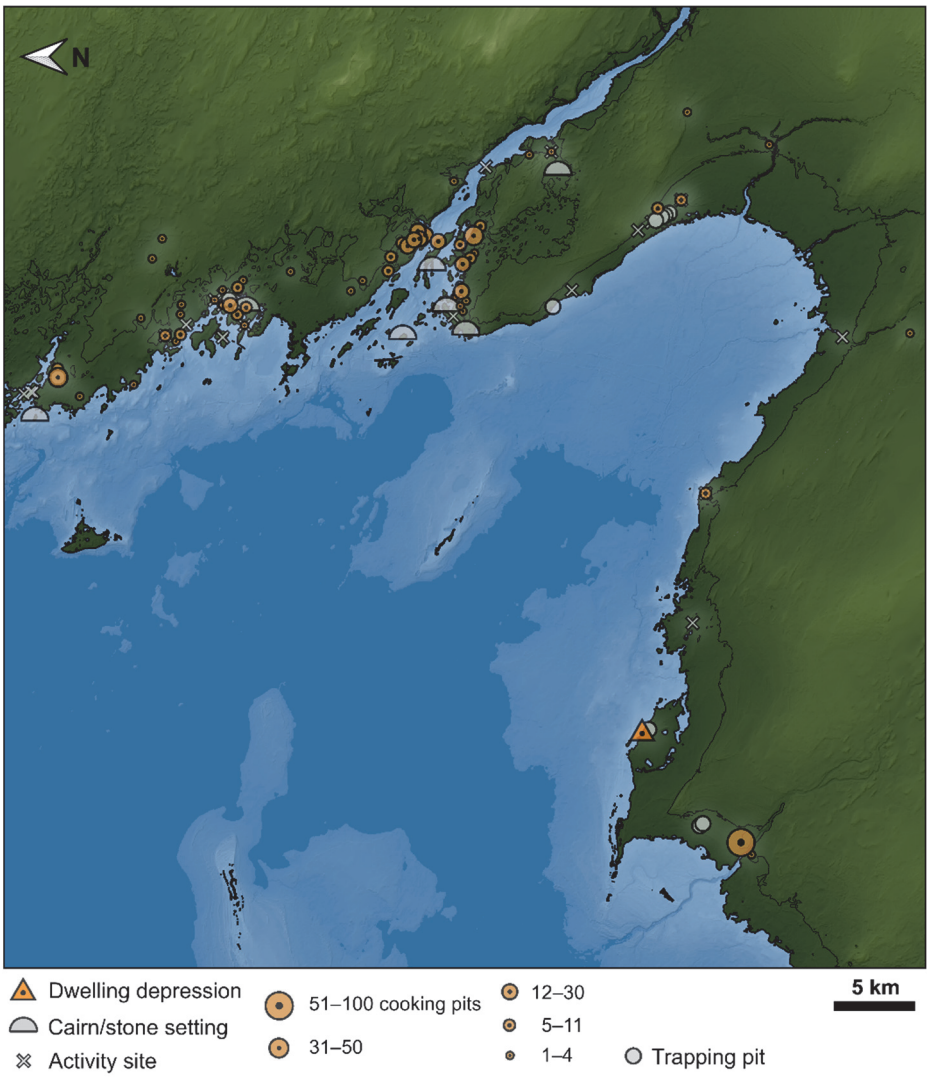

Fig. 32. Cooking pits and other mostly undated remnant sites of the region surrounding the Late Bronze Age Oulujoki River estuary. Shoreline set to 300 BCE, with the higher contour set to 1200 BCE. DEM by NLSF.

knowledge, and the establishment of friendships. In this light, the distribution of bronze could have gone hand in hand with more vibrant processes than systematic exchange of goods (see Chapter 3.4). Yet, we should note there is an opposing possibility. Anthropologist David Graeber described the Axial Age as 'the age of materialism,' which is characterized in the Eurasian-wide written record by an ideological pursuit of profit and related philosophical theories about the essence of substance (Graeber, 2011: 237–250, 297).

Table 5. Radiocarbon dated cooking pits in the two regions (from Paper II).

Site	Cooking pits	BP	Calibration (2 sigma)	Sample ID
Kemijoki River region				
Rovavaara 6	Up to 40	2550±100	893–405 calBC	Hel-3825
Kiimamaa	1	2210±100	508 calBC – calAD 15	Hel-3236
Korkiamaa 3	1	2000±80	202 calBC – calAD 213	Hel-3824
Kortejärvenkangas	1	1610±80	calAD 252–606	Hel-3826
Oulujoki River region				
Hangaskangas E	2	2460±30	758–416 calBC	Ua-45452
Metsokangas	1	2450±70	769–405 calBC	Beta-183716
Jauholaarinkangas	18	2360±60	752–234 calBC	Beta-14101
Sanginkangas E	29	2185±30	360–173 calBC	GrA-63522

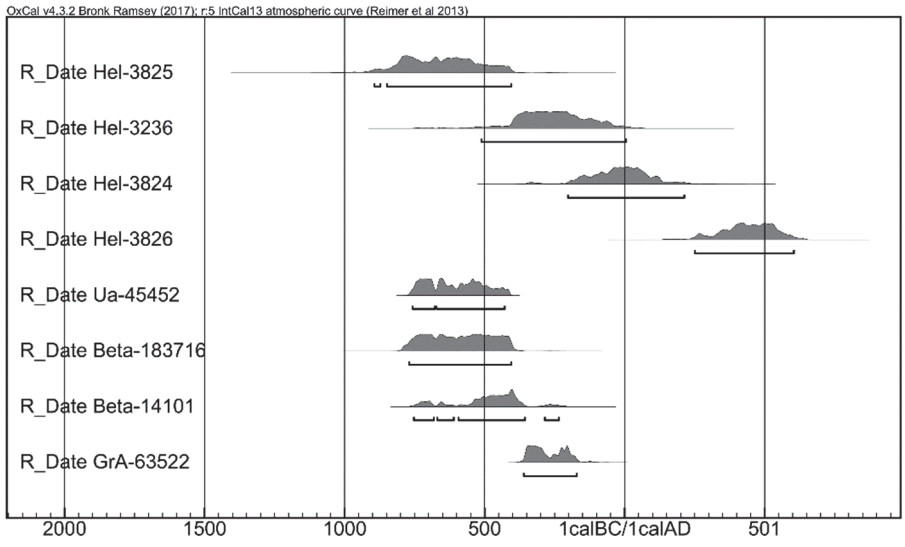

Fig. 33. Radiocarbon dates of Table 5 calibrated in OxCal v4.3.2.

The adoption of iron from local perspectives: new divergence part 1

The beginning of the Early Iron Age is somewhat difficult to ascertain. Currently, even our understanding of early local iron use is based on only five sites. Starting from the north, the first two are burial cairn sites, Itärova [240010062] and Länkimaa 1 [240010048], both containing two small cairns or stone settings. Excavations conducted in 1992 revealed only a few iron objects enclosed within them. Evaluating the chronology of such cairns is difficult as it is based mostly on the dating of optimal landscape conditions, which again relates to shoreline chronology. Between 200 BCE and 100 CE, the locales of the two burial cairns of Itärova emerged, guarding the inlet to a sheltered cove. This landscape condition suggests a hypothetical date of the burials. Within the smaller of the two structures, an iron object, possibly a mangled and twisted spearhead or blade [KM 27702:1-3], was recovered. The larger cairn was left unexcavated (Krankka, 1993).

The rest of the early iron objects found in the region date more likely to the earlier half of the Middle Iron Age. On the same cove as Itärova, the burial cairns of Länkimaa 1 could have been erected on an emerging cape as early as 150 CE, although this may have also taken place later. Even around 550 CE, the brackish water outflow of a small river estuary was located only 100 m away. Along with two bronze brooches [KM 27701:1-2], two iron objects were excavated from the Länkimaa 1 cairn: an arrowhead and an unidentified corroded artefact [KM 27701:3-4] (see Eskola & Ylimaunu, 1992).

While the two previous sites may contain the earliest traces of iron, the most informative site in this regard is Rakanmäki [851010002]. The site is a large activity and burial site, with cairns located in a boulder field overlooking the lower activity site (see Fig. 37). The activity site, a 270 x 100 m patch of sandy northbound slope, was at the time of excavation delimited by burnt rocks in furrowed tracks. Some 5% of the activity site was excavated by archaeologists Markku Mäkivuoti and Pentti Koivunen in 1985–1987. The excavations managed to reveal not only nearly 20 fireplaces and pit hearths, and a possible 2-m wide circular remnant of a structure, but also several artefacts suggesting on-site metal working (see Mäkivuoti, 1988).

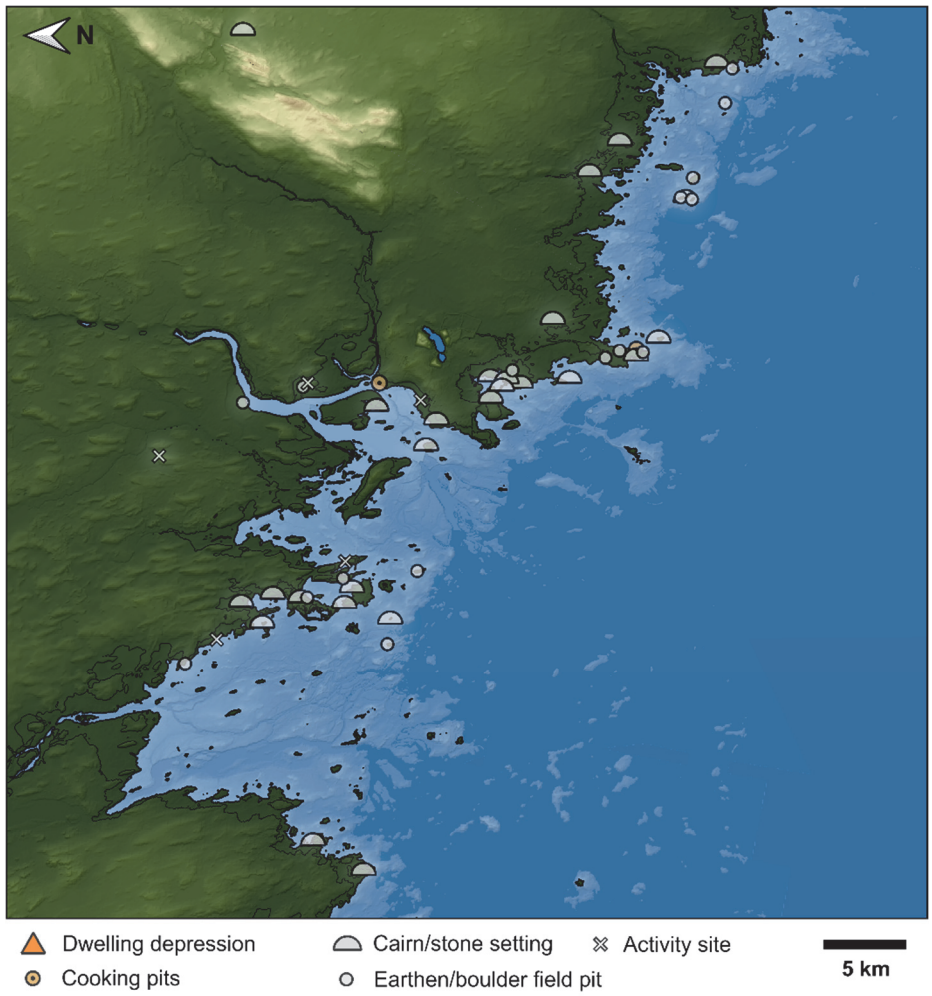

Fig. 34. The archaeological record above the Middle Iron Age (300 CE) shoreline in the region surrounding the Kemijoki River estuary. Higher contour set at 300 BCE. DEM by NLSF and Landmäteriet.

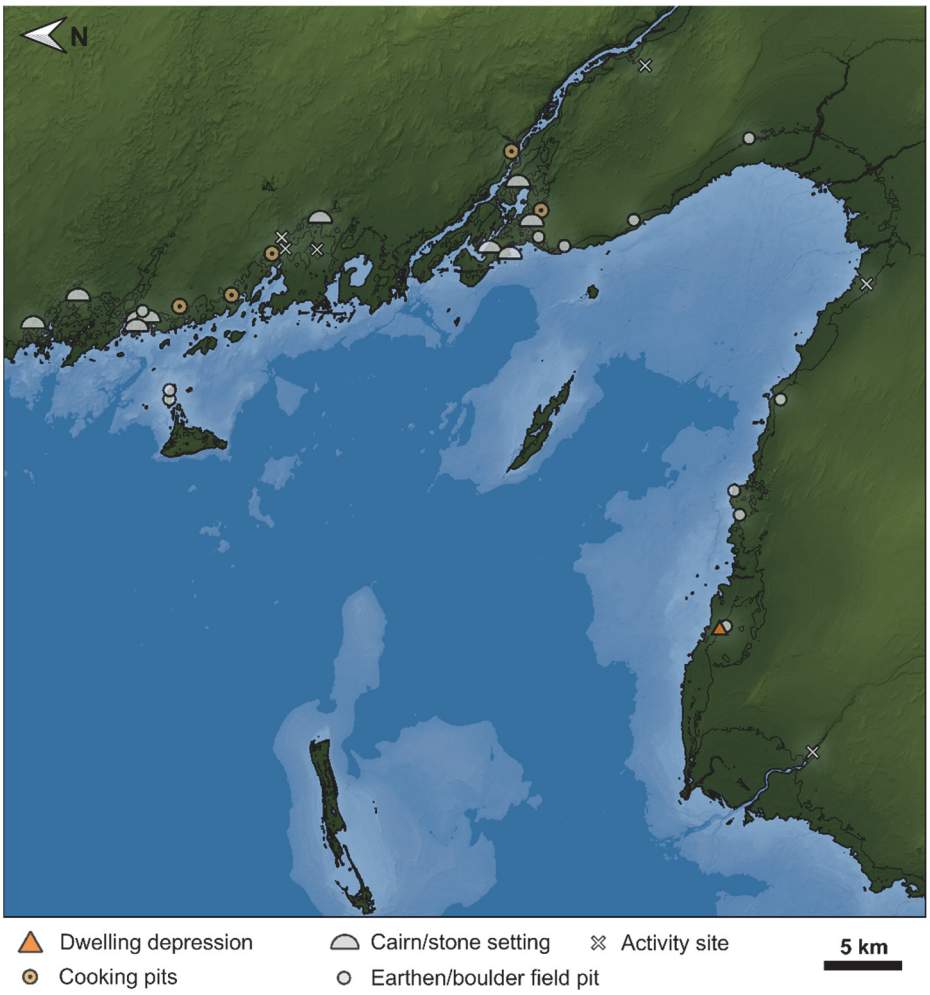

Fig. 35. Middle Iron Age archaeological records of the regions surrounding the Oulujoki River estuary. DEM by NLSF.

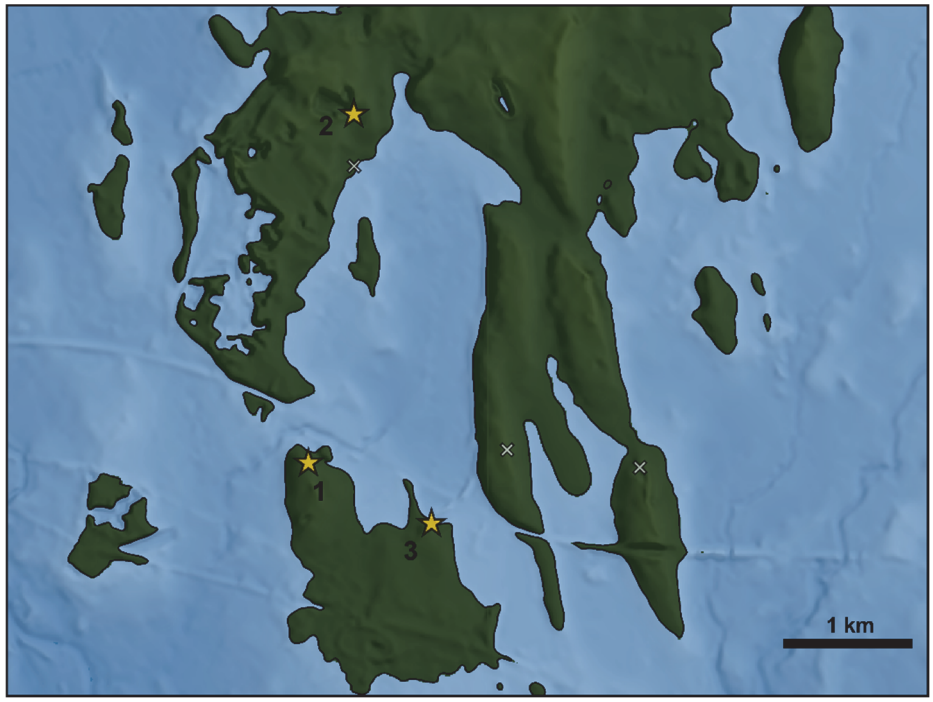

Fig. 36. The remnant-sites of the local community of Laivajärvi, Tornio in focus. The numbered sites mentioned in the text are 1. Rakanmäki; 2. Susihaudanmäki; 3. Lapinkula. The map is orientated to the north. DEM by NLSF.

These artefacts include 4.3 kg of slag, the clay lining of bellows, and several clay crucibles with traces of smelted copper. Based on the radiocarbon dates collected during the excavations the site was occupied between 100–300 CE. A lone spade-shaped iron bar [KM 24206:1] originating from Jämtland in the mountainous region west of the Bothnian Sea indicates the extent of material contacts.

Rakanmäki was located on the northern shore of either an island just off the coast of a bay or an emerged cape formation enclosing a sheltered cove. A similar activity site, Susihaudanmäki [1000000994] (Fig. 36: #2), is located on the northwest shore of the cove less than 3 km from Rakanmäki. Although Susihaudanmäki remains unexcavated, slag was found during its preliminary survey. The two sites indicate a complex set of metallurgical activities, both iron and copper working. Although the intensity of the activities is arguable (see e.g. Kuusela, 2013: Appendix 15) partly due to the exploratory nature of the field

research, the fact that these activities took place here is revelatory in understanding the local community. Additionally, at least two stone box furnaces where iron ore was processed have been found 100 km inland along the Kemijoki River (Kotivuori, 1996b: 108–111). The prominent cairns and a so-called stone pentagon of Rakanmäki (Fig. 37) and the monumental cairn in nearby Lapinkula [1000000993] (Fig. 36: #3) intertwine these metallurgical activities with an expression of identity which we will discuss shortly.

Contrasting the emergence of iron in the north with the existing record of the Oulujoki River region shows a completely different scene. In fact, the earliest iron objects that have been found here date later, 200–500 CE, and were found on a single site. They were discovered in excavations by Markku Mäkivuoti in 1987 and 1988. There are four burial cairns at Välikangas [564010022]. Inside them were at least 12 burials, nine inhumations and three cremations, and an assemblage of iron and bronze objects (Mäkivuoti, 1987; 1989). The rich materiality included at least two swords, several axes, spearheads, an iron seax, bronze rings, and several other metal objects [KM 23911:1-35, KM 24597:1-48]. Over 50 metal artefacts were discovered altogether. The dating of the site relies on the typologies of these artefacts (see Mäkivuoti, 1996), since radiocarbon dating of charcoal samples resulted in unlikely medieval dates. Thus, the dating of the cairns remains suspect, but if the chronology proves erroneous, it is unlikely that the site is older than what has currently been interpreted.

The burials of Välikangas are the only evidence for the use of iron in the Oulujoki River region until after the Mid-Iron Age (300–800 CE). The evidence suggests that iron objects were mostly acquired through material contacts, since both the large number of iron objects and their delicacy should have left some traces of local metal working. Additionally, Mäkivuoti identified several Baltic stylistic traits in the find assemblage of Välikangas (see Mäkivuoti, 2006), suggesting the import of weapons and high value objects. On the other hand, further north in the Kemijoki River region, experimentations with local iron working were undertaken, apparently with both imported raw material ingots and lake ore used in local production. The relatively low quantity of iron objects found in the northern region certainly suggests that such a small quota could have been produced locally.

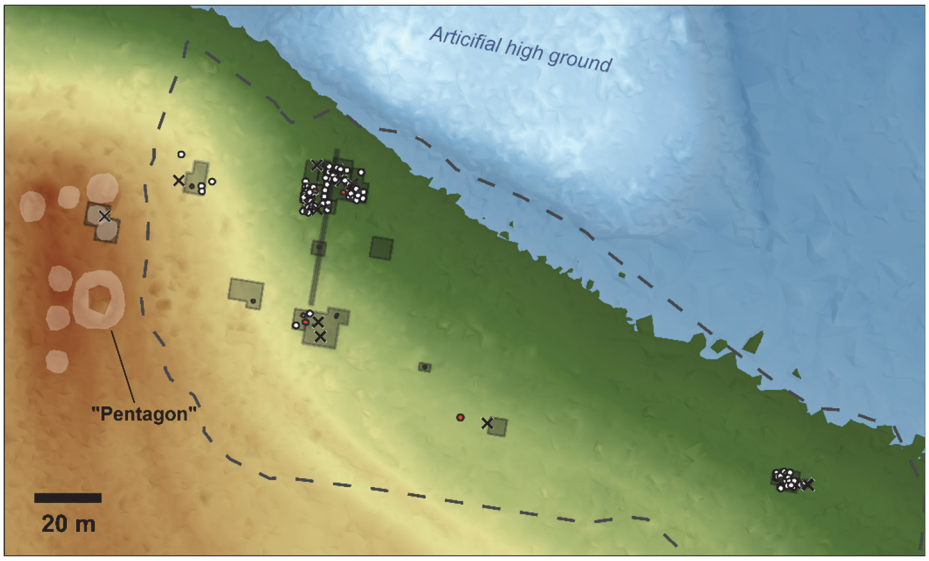

Fig. 37. The Iron Age activity site of Rakanmäki with burial cairns and stone pentagon, and also metal objects (X) and slag finds (white dots). Grey rectangles represent excavated areas, and dashed lines show the extent of burnt rocks found in furrows. Shoreline placed at 300 CE. The map is orientated to the north. DEM converted from lidar (0.5 dots/m^2, NLSF, CC BY 4.0).

Monumentalism of Mid-Iron Age burials: new divergence part 2

Finally, the last theme concerns the burial structures of the Middle Iron Age, partly overlapping with the previous theme. The focus is on the large burial monuments of the 1st millennium CE. In our two study regions, these are only relatively monumental. The burial structures of the Bothnian Arc cannot really compete with the monumentalism of certain examples further south, such as the burial mounds of Uppsala, the large Neolithic dolmens of Southern Sweden, or the Bronze Age monuments of Satakunta.

Fig. 38. An unusually large cairn in the context of the Iron Age Bothnian Arc, within a boulder field at Kirnuvaara. Author camouflaged on the left as scale (photo by Ville Hakamäki).

Yet, the burials considered here are such that they would have garnered considerable attention in relation to the bulk of the contemporary local graves. Although there are several periods where monumental burial traditions could be compared, beginning from the Late Neolithic/Early Bronze Age long-cairns, few such remnants are accompanied with a reliable chronology. The dating of individual cairn sites through shoreline chronology alone is extremely uncertain. But since it seems that the burial cairn tradition ended at the latest by 500/600 CE (e.g. Kuusela, 2013: 135–136), and the terminus post quem of the monuments in question is mostly after 1 CE, this gives us a rough 500-year window in which to perform a comparison.

The current view is that the cairns of Välikangas represent the last phase of the northern burial cairn tradition. On the Northeast Arc, only three sites with possible burial cairns—Lapintuulen risteys [240010130], Urheilukentän eteläpuoli [1000023387], and Kyöpelinharju [139010030]—lie at lower elevations, indicating that they could not have been constructed until after 400 CE. None of these cairns,

five in total, have been verified as burials, and in the most recent survey, three of them could not be located at all (Hakamäki 2013: 19). As such, the current study maintains this chronological view. But what can we determine from the perceived differences between the highly visible cairn tradition of the northern region in comparison with the southern region?

As with our interpretation of contemporary iron use, the cairns that apparently lie within the 100–500 CE temporal range also indicate local variation, but in a different way. It can be argued, again, that these burial structures are expressions of identity. The cairns in the southern region are comparatively modest, while containing an intricate inner structure of multiple cists (Mäkivuoti, 1996). The cairns in the northern region, e.g. in Rakanmäki, Lapinkula, and Kirnuvaara [751010037] (Fig. 38), seem to be the opposite. They are prominent on the surface, but although several have been excavated, these have contained no clear inner structure (see Mäkivuoti, 1989; Eskola & Ylimaunu, 1992; Krankka, 1993). It is as if the message they emit is decidedly different. The large graves might communicate more to outsiders, who marvel at the size of such monuments, even though effort has been spared when designing their inner composition. Should two large stone monuments, so-called pentagons, located in Rakanmäki and Susihaudanmäki, be contemporaneous (see Mäkivuoti, 1988; Saloranta, 2011), the imposing outward-directed message is even clearer. In contrast, the less overt yet delicate cairns in the south have more meaning within the local community and to those with knowledge of what lies inside the cairns.

This interpretation can be combined with the apparent logic regarding iron procurement. The communities in the southern region seem to have relied on outside sources for their metals, while in the north the experimentation with indigenous iron working indicates that the communities strived for self-sufficiency. While the imported Norrlandic iron bar argues against total independence, the question remains, why was it not expended in production but remained as an unprocessed object. Was its preservation deliberate, and was it chosen not to be used? Such a decision could have communicated a symbolic separation from the rest of the Baltic Sea Rim, a separation, which the contemporary community on the southern coastal Oulujoki River region hardly seems to have maintained. If this were so, the political logics of the two coastal riverine communities would have been at odds, yet again suggesting separated group identities.

This divergence seems to have been followed by shared action that is expressed by the “Mid-Iron Age forager gap”. The material gap seems to result from the replacement of stone and clay by iron, and the lack of preserved symbolic

expressions, such as burial monuments. I suggested earlier in Chapter 6.1 that this development may indicate a period of political anarchism, or the active negation of leadership roles. Whether this interpretation remains valid, we shall see.

7.2 Returning to the Late Bronze Age community of the Oulujoki River estuary (Paper I)

There is a standing joke that the archaeological site of Tahkokangas in Oulu must be among the most publicized archaeological sites of which nothing can actually be said. The site, consisting of twelve unassuming stone settings, was partly excavated by Jari-Matti Kuusela in 2011. The find assemblage consisted of a single fragment of a flint object and a cache of about a hundred spent assault rifle shell casings hidden under a rock and abandoned after military exercises (Kuusela, 2012). Subsequently two master's theses were produced, first by Ville Hakamäki (2012) and the second by myself (Hakonen, 2013). Also Kuusela himself briefly discussed the site in his doctoral dissertation (Kuusela, 2013: 40–42). Three research articles were eventually published, most recently Paper I (see also Väänänen, 2012, and Hakonen, Kuusela, & Okkonen, 2015).

Tahkokangas offered me the first real glimpse into a prehistoric community. As we are nearing the end of this foray, it is well that we should return to the subject, if only to see how my own view of the site and my previous interpretations align with what I discovered in composing the rest of the papers.

We can still be fairly certain that the stone structures—some of which are stone settings consisting of only a few layers of stones, while others are more prominent stone enclosures with central cavities (Fig. 39–40)—are burials. This comes down mainly to their morphology, as shown by Ville Hakamäki in his master's thesis (2012). Similar stone settings have been excavated in the northern coastal region of the Gulf of Bothnia, and also in interior regions of both Finland and Sweden (see Paper I). As I noted in Chapter 5, shallow stone settings are a frequent remnant-type in the Bothnian region, and when excavated several have turned out to contain burnt human bone. Most, it can be concluded, contain nothing, as organic materials enclosed within have decayed.

Fig. 39. Stone encirclement settings in a boulder field at Tahkokangas (photo by author).

An unfurnished stone setting or cairn, therefore, requires structural cohesion for it to be interpreted as a burial structure. First, the intentionality of otherwise ambiguous stone structures can be assessed by comparing the stones that form the outer edges with both the façade and the filling. In the vague cases, excavation is the only way to assess structural integrity. Should the stones contained within be of random size and composition, it could be argued that the setting is a human-made stone heap or a natural occurrence. If the stones are in cohesive layers, yet their composition changes, with the largest stones on top and the smallest at the bottom, the likeliest explanation is a naturally sorted geological formation. But if clear structure can be ascertained, such as a central cist formed of stone material deviating in size or composition from the surrounding filling, or if there is deviating material such as larger stones outlining the structure, a case can be made for its function as a burial.

Uncertainties in such interpretations should not be disregarded. Could such stone settings be simply storage pits, perhaps for animals that were caught and butchered on location? Anything is possible, especially with mute and unassuming archaeological remnants such as the stone settings of Tahkokangas. But, with the clear morphological connection to established graves, and their dissimilarity to

verified storage pits, especially their extent and depth (see e.g. Valtonen, 2006), the burial interpretation remains likelier in my opinion.

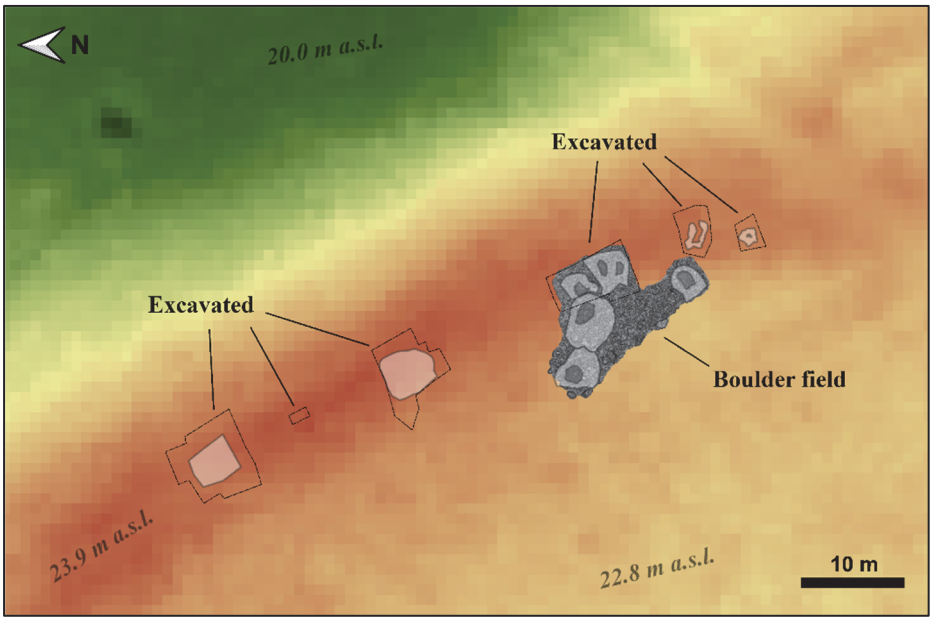

Fig. 40. The stone settings and boulder field at Tahkokangas on a DEM converted from lidar (0.5 dots/m², NLSF, CC BY 4.0, conversion by Janne Ikäheimo). Spatial data based on Total Station measurements and terrestrial laser scanning.

But what about the site's context? Has something changed in the interpretations of Paper I in the six years since co-authoring it with Ville Hakamäki and Jari-Matti Kuusela? For one, the shoreline displacement chronology of the paper used the median value published by Vermeer et al. (1988), along with error margins of the high and low deviations. The local shoreline of 500 BCE was defined at 19.5–21.9 m a.s.l., with a significant 2.4 m error margin. The 23 m a.s.l. shoreline was dated within the range 600–880 BCE. Paper II allows us to narrow down these evaluations, by applying Kääriäinen's (1982) high values to the equation. The amended chronology is practically in line with the high-end deviation presented in Paper I when reporting elevations, and low-end deviation when reporting calendar years. Thus, we can realign the originally depicted landscape process: the island emerged ca. 860 BCE; the stone settings could have been constructed ca. 750 BCE at the earliest; the island remained the farthest of the archipelago until ca. 560 BCE;

the island fused with a larger island ca. 200 BCE; the larger landmass merged with the mainland ca. 60 CE.

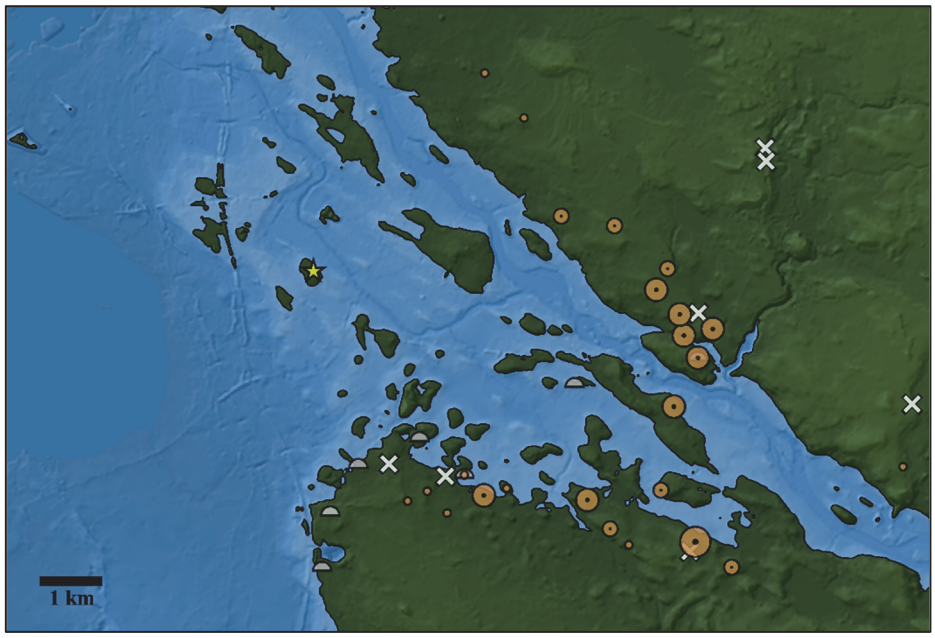

Fig. 41. The island of Tahkokangas (star) and the Oulujoki River estuary, with cooking pit sites (circles, increasing in size depending on the number of pits, from 1 to 46), stone cairns and settings (semi-circles), and activity sites (X) lining the coast. Shoreline set to 300 BCE. DEM by NLSF.

A subsequent analysis in Paper I regarding the distances from cooking pit sites to the shore has a shoreline set to 20 m a.s.l. or 310 BCE. I was laboring under the assumption that the cooking pits had a clear connection with the shoreline, which indeed they do: their distribution matches the shape of the Late Bronze Age Oulujoki River estuary (see Fig. 41). However, I saw the cooking pit record more statically than I now understand it. I was searching for an average distance from the shore to the cooking pits, which I hoped could have clarified their dating. Now I think that in order to do that, a systematic radiocarbon dating regimen should be implemented. Looking through the lens of the reiterated chronology of Paper II, it becomes clear that all cooking pits in the Oulujoki River estuary could have been established as early as 500 BCE. In fact, out of 194 cooking pit sites registered in

Finland by 2016, all but two (both individual pits on the Iijoki River estuary) could have been established as early as that or prior to it. Nevertheless, the admittedly unrepresentative radiocarbon date record indicates a temporal range of 800–200 BCE for cooking pit clusters. This is, surprisingly consistent with Paper I, but will remain suspect until a systematic dating regimen is implemented.

What I did not truly comprehend was the big picture. We painted a smaller picture of a local proto-elite, whose leadership dictated surplus production in order to gain access to the European wide network of bronze distribution. According to this thinking, the aim of the proto-elite was to consolidate their power and that of their successors by differentiating the burials of those on the higher rungs of society from those on the lower rungs. By controlling the strategically located river estuary, with both practice and symbolism, the proto-elite also controlled the material influx to the region, which resulted in a strengthened hierarchy. Disturbances in the European material networks (see Kuusela, 2013: 128–135) hindered the consolidation of power, leading to a collapse of productive activities and the abolishment of the hierarchy, leading to political anarchism.

The main thing that has changed in my thinking, to my surprise, is that I am no longer quite as cynical. While such an interpretation remains intriguing, the big picture suggests that it may be somewhat exaggerated. This was my own making: in commenting on the first draft Dr. Kuusela actually suggested that I tone down the rhetoric regarding hierarchical differentiation within the community.

What I also did not know at the time is that such occurrences of increased production had happened previously, multiple times during at least some two millennia leading to this period (Paper III). I suspect that such events may have even been recurring, but due to the nature of the archaeological record, we simply do not have sufficiently detailed chronological resolution nor evidence of all pertinent activities to identify such short-term processes. Still, it can be argued that the circumstances of the local community were not novel. In fact, it is possible that the previous experience of increased production lived on as cultural knowledge.

Anthropologist David Graeber argued that in various egalitarian societies the basis for anarchism lies in counterpower, or the active resistance to hierarchy. Counterpower, in this context, arises from the historical memory of instances where ambitious individuals tried to centralize power unto themselves, with negative consequences for the rest (Graeber, 2004: 21–37). In the Late Bronze Age Bothnian Arc, similar instances had already occurred at least from the Middle Neolithic onwards. These events had undoubtedly been meaningful, and as such their cultural legacy would have been carried on in oral history, folklore, and mythology. Thus,

with some recollections of prior instances likely to have been transmitted as cultural knowledge, the local community was, in my current thinking, more inclined to counteract hierarchical relations from within as they were formed, instead of hierarchy being a matter-of-fact development to be disrupted by outside influence. Archaeologist David Wengrow and Graeber (2015), have argued that such tug-of-war between hierarchical and egalitarian relations within communities was in fact a common occurrence even during the Paleolithic.

Nevertheless, some form of leadership and consolidated decision making seems to be evident based on the, let's say, "industrious" practices related to the cooking pit clusters. Kuusela and Jari Okkonen have previously envisioned feasting as the main reason for such practices (see Kuusela & Okkonen, 2013). In the connected world of 700–300 BCE, this interpretation may fit very well. The world at this moment was uniquely global, at the very least from the Eurasian standpoint. At least three continental bronze-related material networks were maintained in Europe, as discussed previously: networks of celt axe technology, of Mediterranean bronze, and of eastern copper. As said before, the first and possibly the third of these, may have extended as far east as China. Also, the Tin Isles of Britain were already connected with the Mediterranean network.

If we adopt Kuusela's interpretation of cooking pit related feasting, we may better understand the intricacies of the abstract material networks. After all, feasts are often associated with gift giving, which may in all likelihood supplant my original conception of trade (embedded in Paper I). This is partly why I grappled with the subject of prehistoric trade in Chapter 3.4. The acquisition of bronze may have actually been a byproduct of maintaining relationships with other, even distant, communities.

With a more up-to-date perspective, in line with the interpretative key of Paper IV, I see the burial structures of Tahkokangas mostly as signs of respect within the community. Constructing them was not all that laborious. I should know since I took part in deconstructing some of them. They were already located in a natural boulder field, so the material did not need to be hauled from afar. While I do still think that wooden frames and posts, perhaps even roofs, may have been used in connection with the burials (see Hakonen et al., 2015), due to the size of the stone settings, this would not have made the job significantly more laborious. Compare this with the monumental burials elsewhere—in Neolithic Southern Scandinavia, in Bronze Age Satakunta, in Iron Age Västernorrland, or even in the Mid-Iron Age Kemi-Tornio region—where a workforce needed to be organized specifically for the purpose, and it shows that the relative labor involved in the establishment of

Tahkokangas does not necessarily symbolize excesses of power relations, but instead suggests more voluntary action by a community respecting their valued dead.

The question remains, who were the buried individuals? Why were they afforded such burial structures, while others arguably were not? Were these the political leaders or were there other roles belonging to different spheres of existence within the community which warranted such burials? All in all, how multivalent were the local burial practices? These questions I cannot answer. One of the most intriguing aspects of archaeology, I would argue, is that it does not pretend to know all the answers but leaves something for the imagination.

8 Summary of results

This chapter presents short summaries of the main results of the research articles combined with the related interpretations presented in this synthesis.

Paper I

In certain contexts, local communities are discernable in site assemblages within the archaeological record. One way to study them is to collate the evidence from several nearby sites where subsistence and ritual activities were practiced alongside other activities, e.g. relating to regional politics. Applying site assemblages instead of individual sites as the unit of analysis multiplies the modes of existence discernable in the context, while permitting local differentiation. In this case study focusing on the Late Bronze Age Oulujoki River estuary, local political activity concerning ambivalence towards the cooking pit related productive activities and a resulting counteraction against hierarchical social arrangements can be interpreted.

Paper II

As such archaeological site assemblages are embedded in the geography, the reconstruction of contemporary landscapes is of paramount importance. In coastal Northern Fennoscandia, this reconstruction relates mostly to post-glacial land uplift. An existing chronology was refined by evaluating it against the contexts of archaeological radiocarbon dated samples. Several inconsistencies regarding regional uplift rates were identified. Some of these can be traced to erroneous basin isolation dates, but some may be related to other issues, such as changing wind patterns. Additionally, three tiers of association with the coastal shoreline were established from the coastal archaeological record. Dwelling structures are most likely to occupy a place within 50 m of the shoreline, while the location of burial cairns seems much more varied. Cooking pits on the other hand are more likely to have been used at a considerable distance, more than 200 m, from the shoreline, revealing their dating with shoreline displacement chronology to be extremely inaccurate.

Paper III

Understanding local communities requires knowledge of both their history and their wider context. The long-term prehistory of Central Fennoscandia contains an obvious material divergence between the north and south. Two different overall cultures seem to have developed, with the record of Southern Finland sharing an underlying logic with Southern Scandinavia, while in Northern Fennoscandia a logic of its own was maintained. Interpreting the divergence as a regional shift in subsistence strategy is simplistic and recurringly at odds with the archaeological record. Instead, considering the development in terms of subsistence *ideology* allows more variety in how communities operated. The analysis suggests that while in the northern procurement ideological context some laborious activities and agricultural experiments were undertaken, they were subsequently abolished, signifying active resistance to their consolidation. In contrast, the southern production ideological context saw such activities persisting without similar periods of abatement, but rather such practices accumulated. This observation may to a large extent explain the differentiation that is reflected in the long-term record, with political organization in the north aligning more with anarchistic relations and hierarchical relations becoming more prevalent in the south.

Paper IV

Shifting the scope to an archival survey of excavated archaeological burial sites highlights shared themes but also reveals local variety in mortuary practices in both previously defined contexts. Applying *respect* as an interpretational key, relevant regardless of ideological context or time, mortuary practices express positive emotional action. Thus, the undertaken actions ultimately represent morally accepted behavior by the enacting communities. With respect as the key, burials in the region are more reflective of the multivalence of practices, instead of the focus being shifted on deviancy from a perceived norm. Both concepts, “deviancy” and “the norm,” have troubled epistemological foundations, and are more in tune with relatively recent times. Similarly, fear of death and the dead, which are often applied in archaeological research as projected interpretations of mortuary practices, may carry with them anachronistic elements. Instead, the shared themes that emerge as more pertinent in mortuary contexts are the agency of fire as well as the agency of geography, which accompany the mortuary record in differing manifestations throughout the long-term past.

Paper V

Finally, zooming in closer to local communities, the focus is realigned on their defining characteristics. In another case study, these characteristics were illuminated by analyzing dissimilarities between two regional archaeological site records. The concept of *living geography* offers a grounded definition for the geographical scale of the analysis, with major river estuaries binding the assemblages. Comparing the continuous records of both regions shows how throughout prehistory material traditions diverged locally. Several active phases of differentiation between local communal identities may have occurred. The interpreted methods for this differentiation range from the choice of stone materials, arrangement of dwelling organization, and, in the extended analysis detailed in this synthesis, the contrasting ways to procure iron and the difference in conceptualizing mortuary structures and to whom they communicate. In the Late Bronze Age, contemporaneously with the intercontinental Axial Age, the two regions apparently undertook more shared activities than in other periods, a development which may reflect a previously hypothesized materialistic phase in human history. In the Mid-Iron Age, after a period of new divergence of local identities, the archaeological record practically vanishes, suggesting a shared phase of politically anarchistic immaterial relations.

The last case study, the collation of local prehistories for a systematic comparison with a focus on disassociations, may offer a methodology for understanding how prehistoric collectives truly functioned, without exposing the subject to the danger of being contaminated by anachronistic concepts. Such analyses may work especially well should their conclusions be cross-referenced with the opposing *community of practice* approach, where the focus is instead on associations that freely cross geographical delimiters.

9 Discussion and lingering questions

On nearing the end of this study, I adhere to the advice of archaeologists Urban and Schortman (2019: 302): do not be afraid to fail. Thus I will ask what I think are important lingering questions, which I do not necessarily have the answers for. But first I will briefly discuss how the archaeological interpretations presented in this dissertation are relevant to our current existence. After that, I will present three issues that have not been resolved, but which require more research and much more discussion, beginning with the validity of the concept of prehistoric society. Other points I am about to raise include two issues of possible bias regarding prior research. Finally, regarding a third issue of bias, I seek to shed light on the faults in my own methodology.

9.1 Reflections of the times

Despite its roots in the past, archaeology is made in the present and accordingly it cannot escape the culture in which it is performed. Structural archaeology went hand in hand with 19th and early 20th century colonialism, while processualism emerged in the 1950s when macroeconomic theory began infusing the language of politics. The matter-of-factness of network thinking has undoubtedly been accentuated by the increasing ubiquity of global trade and the Internet. In this light, archaeology, in particular, can be seen as a reflection of the times, past and present.

On looking back, three of my subjects, labor (Paper III), death (Paper IV), and community (Paper V), reflect some of the sores of modern society. Work and labor are virtues that are often valued more highly than friendships or health. This has resulted in a swarm of recent news describing an overworked and overstressed populace. Since there is not enough meaningful work for everyone, life in such society has turned into a competition. The front-runners, idealized in the media and political rhetoric, are often successful entrepreneurs or CEOs, while their bankrupt competitors, as numerous as the former, are seldom worth mentioning. With competition seeping into the fabric of life, into workplaces, classrooms, and political forums, burnout, the apparent symptom of physical brain damage resulting from social pressure, has become endemic. The unemployed, the aimless, and those who refuse to partake in the never-ending competition are labeled society's refuse, whose infectious passivism has to be curtailed through penance. This was the social tenet of the Finnish government of 2015–2019, a tenet likely to be reinstated after electoral politics has completed another full circle. Composing Paper III in this

political climate undoubtedly affected my interpretational key. I can only assure the reader that I worked hard to maintain objectivity in data gathering, analysis, and my interpretations, but whether the account I produced maintains its veracity, withstanding the criticism it deserves, remains to be seen.

Death, meanwhile, has a hold on us. We have become accustomed to not encountering it in reality. Our society has tried its best to sweep death out of sight. The handling of death has been left to the professionals—nurses, morticians—even though everyone will have to face it in their lifetime. It has been argued that this has increased our anxiety not only toward dead bodies, but also of our own inevitable death (see Rinpoche, 1997/2004). Accepting death and gaining familiarity with it could alleviate many of the psychological problems society faces, but this would require actively transforming our culture.

These problems we face with an infallible trust in the modern governed society to transcend its ills through progress. But in trusting society, we have demolished the community. In sprawling cities, drifting alone in the crowd is already the norm. Should one be so lucky as to have a stable community as a child, that community will invariably break down, with families moving in pursuit of work and childhood friends dispersing in search of education. Adolescents are forced to make decisions regarding their future without the necessary experience or knowledge to navigate the overflow of possibilities. Since work and family are often separated into their own worlds, children can no longer by default follow in the footsteps of their parents, but in the spirit of entrepreneurialism have to reinvent their own lives. This reinvention often necessitates the disintegration of any community the child may have had. In the modern world, the absence of community is supposed to be mended by society taking its place. But the ideal of society's safety-net has been displaced by burdensome bureaucracy. Even without bureaucracy, society may not be all that welcoming, as political vitriol permeates it, assorting soldiers to myopic tribalistic causes, while leaving many in the void.

Journalist and long-time war correspondent Sebastian Junger, in his book 'Tribe: On Homecoming and Belonging' (2016), investigates the breakdown of the sense of community in modern society. In interviewing survivors of major conflicts he found civilians and combatants actually missing wartime because of the sense of togetherness that prevailed during it. Junger argues:

> What people miss presumably isn't danger or loss but the unity that these things often engender. There are obvious stresses on a person in a group, but there may be even greater stresses on a person in isolation, so during disasters there

> is a net gain in well-being. Most primates, including humans, are intensely social, and there are very few instances of lone primates surviving in the wild. A modern soldier returning from combat—or a survivor of Sarajevo—goes from the kind of close-knit group that humans evolved for, back into a society where most people work outside the home, children are educated by strangers, families are isolated from wider communities, and personal gain almost completely eclipses collective good. Even if he or she is part of a family, that is not the same as belonging to a group that shares resources and experiences almost everything collectively. Whatever the technological advances of modern society—and they're nearly miraculous—the individualized lifestyles that those technologies spawn seem to be deeply brutalizing to the human spirit. (Junger, 2016: 93)

There is no telling whether these ills of modern society are allowed to remain. Looking at ourselves through the mirror of the past, however polemical it may seem, can illustrate more clearly how troubled our society has made itself. Perhaps archaeology has its benefits in not only diagnosing our present pathologies, but also in coming up with actual solutions for mending our shared issues.

9.2 Should archaeology forgo society as a unit of analysis?

In the theoretical discussion, we already discussed some of the issues in using society as a group concept in prehistoric archaeology. The question remains, what is society? Comedian, and the host of the Daily Show, Trevor Noah asked this question in the initial days of the 2020 George Floyd protests in the United States. Some of the protests had erupted into violence, destruction, and looting. On pondering the underlying roots of the anger, Noah's conclusion was that society is a contract. When the contract is broken by one party, such as the police by using violence as the default way to *police* minorities, the obligations of the violated party are nullified. According to Noah, instead of condemning looters, the question should be, why not loot? If the contract is broken, why uphold it unilaterally?

Trevor Noah's acute and timely understanding of society stemmed from his upbringing in a mixed ethnicity family in the racist apartheid era of South Africa. Here, during much of the 20th century, the state led society was segregated into two tiers: black and white. With its basis in white supremacy, the policy of apartheid, literally segregation, was eventually peacefully abolished through negotiation. In

this context, a badly written contract can be said to have been renegotiated into a more inclusive one.

Far from expressing a novel concept, what Noah articulated was social contract theory. Social contract is an early sociological concept, deriving from Thomas Hobbes's treatise the *Leviathan* (Hobbes, 1651/2009). The context of Hobbes's work is the 17th century England, rife with unrest and power struggles. The idea of social contract, not initially named as such, pertained to a political contract between the state and its citizens. In this arrangement the citizenry, usually implicitly, grants the state the authority to make decisions on its behalf, and in return the state protects the citizenry from harm.

The concept originates from an era of civil war, which Hobbes sought to end by defining the political power structure, so that power resides solely with the established state, and that state together with its citizenry constitutes a society. Establishing the concept of society was an attempt to remove power vacuums, which had caused continuous internal competition leading to a cycle of renewed civil wars (see Midgley, 1983; Latour, 1993). After Hobbes, Jean-Jacques Rousseau named the adhesive that forms Hobbes's society as social contract (Rousseau, 1762/2012). Social contract has been accepted as a basic concept in understanding the governance of a human society, with an array of thinkers expanding on and reinterpreting the original concept.

I became acquainted with social contract theory through my wife's study of the Finnish canine police culture. She understood the adhesive of the team as a form of interspecies social contract. The more we dwelled on this in our yet-to-be-published joint paper, the more applicable social contract appeared in the interspecies context due to the imbalances of decision-making ingrained within the original concept.

Hobbes's Leviathan is a metaphor associating society with the biblical sea-beast, understood in Hobbes's times not to have been a 'single creature but a coupling of many together into one' (Mintz, 1989). In opposition to society, or the good Leviathan, was anarchy, the evil Behemoth, a beast intent on destruction and slaughter. I mentioned previously that I am less cynical than I used to be, but in Thomas Hobbes cynicism prevailed with a vengeance. In Chapter XIII of Leviathan, 'Of the Naturall Condition of Mankind, as concerning their Felicity, and Misery', Hobbes famously argued that without the state, people are equal. This was the 'natural condition of mankind' in Hobbes's mind. In such a setting, since people are equal, they are in constant war with each other, because supposedly without state control, resources are invariably scarce and competition for them results in violence. And in such a constant civil war, meaning anarchy,

> …there is no place for Industry ; because the fruit thereof is uncertain : and consequently no Culture of the Earth ; no Navigation, nor use of the commodities that may be imported by Sea ; no commodious Building ; no instruments of moving, and removing such things as require much force ; no Knowledge of the face of the Earth ; no account of Time ; no Arts ; no Letters ; no Society ; and which is worst of all, continuall feare, and danger of violent death ; And the life of man, solitary, poore, nasty, brutish, and short. (Hobbes, 1651/1909: 97)

This account is the foundation of society. Without society, which Hobbes termed as the Commonwealth, meaning the abiding cooperation between citizens and the state, there was only eternal torment, a hell on earth. While Hobbes already used the word *society,* in his time it did not mean group organization, but instead it meant civility and friendliness. It derives from the Latin *socius*, companion or associate. So basically Hobbes argued, that prior to friendliness, there already was the state, and thus savages, as was the early colonialist term for Non-Europeans and Non-Christians, had not yet invented friendliness.

Hobbes, one might say, benefited from the lack of reference. Ethnographies which grasped for objectivity would be compiled two to three centuries later. Society, by then a firmly rooted organizational concept in all of humanity, would be projected even onto people living in the absence of state. This way, people could be corralled into ethnic groups, who could be theorized to share values and organization. But one cannot escape the correlation in what Hobbes saw as anarchy and what Adam Smith saw as barter (see Chapter 3.4). Both are assumptive primordial systems invented by Enlightenment thinkers. Society, as applied in anthropology and archaeology regarding stateless groups, is as if it were held together by the state, but the state is absent. If there is no state to govern society, especially in cases *where the state has never been*, does society remain? I would argue that if we take society as a form of organization, then no; but if we take it to mean friendliness, companionship, and association, then obviously yes. However, the contractual meaning of society has overtaken the earlier one.

How does the contractual notion of society, derived from Hobbes's commonwealth, translate to prehistory? It would be well to reconsider whether it relates at all. The term has become an abstraction, especially useful due to its indefinability. The concept of prehistoric society is usually shorthand for something much more complex and multivalent. In reality, I argue, the concept hides within it an assortment of communities which may have been vastly dissimilar. Not only that,

but obviously communities consist of complex individuals, who themselves are not singularities.

In my view, this is one of the major flaws of Paper III. In the paper I grouped together a bunch of communities who due to the sheer geographical expanse may have had nothing to do with each other, except that they seem to have shared some ideological roots. This could be argued as an eventuality in large regional studies, especially when timeframes span millennia. It may partly reflect reality, since culture as a context, the amalgam of shared mental models and meaningful materiality (see Chapter 3.5), is not as easily subjected to the same critique as the concepts of pre-state society and economy prior to currency. But still, the basis for the binary group categorization in Paper III lies closely akin to the concept of prehistoric society. While there are benefits to such an approach, more theoretical diligence is needed to verify whether the facts on the ground actually support a binary division. In my mind, a renewed focus should be applied to investigating prehistories on the local level, as exemplified by Paper V, yet, preferably with shorter overlapping temporal scales, which, when such studies are eventually collated, would allow for a wider sense of diversity.

9.3 Error margins in radiocarbon dated gyttja stratigraphy: categorical or systematic?

Land uplift curves drawn from geological basin isolation dates contradict the curves based on archaeological benchmarks (see also Lahtinen et al. 2016). In Paper II, I ambiguously described the possibility of a categorical error, meaning that different sampling criteria could have caused the discrepancies. It is also possible that the issue actually represents a systematic error in basin isolation dates, where the ^{14}C contents in stratified gyttja are not static but have leached vertically to a varying degree. This issue should be studied further, since basin isolation dates, and especially the radiocarbon dating of stratified gyttja, provide the bulk of the chronological data for not only geological studies of land uplift, but also for pollen studies that have been used for reconstructing the past climate (see Chapter 4.1). The dating of gyttja thus represents a critical juncture, where related errors are carried on into wider fields of research. The method should be put under scrutiny to see whether the problems persist. If there is a systematic error, it should be noted that the chronology used in the research of the past climate could be affected.

Comparing geologist Mattias Lindén's (2006) apparent land uplift curves with archaeological benchmarks and the relative uplift between regions indicates that

basin isolations seem to date systematically older than they should. According to Lindén's research, the shoreline of Norrbotten in 1050 BCE resided at the current 23 m a.s.l. level, while in 3050 BCE the shoreline was at 39 m a.s.l., and in 4050 BCE at 60 m a.s.l. The elevation difference in 3000 BCE between Lindén's curve and my extrapolated curve, based on archaeological benchmarks and relative GPS-measured rates (see Chapter 4.3), is no less than -14 m. The same issue is evident in Paper II, when comparing the uplift rate in the region of Rauma, based on geological benchmarks (see Vuorela, Penttilä, & Lahdenperä, 2009: 88, Olkiluoto) to my extrapolation, resulted in a difference of -2.6 m in 3000 BCE (Paper II: Fig. 6). The same is reflected by the basin isolation dates for the Kokkola-Kruunupyy-Luoto region, as the difference between the most fitting isolation curve (Vuorela et al., 2009: 80, Case1 V2) and the Kääriäinen high curve, which was adopted as the best fit for archaeological benchmarks, is -1.5 m in 1000 BCE.

Contrastingly, in two instances in Paper II, land uplift curves drawn from basin isolation dates indicated uplift that was too rapid, leaving archaeological radiocarbon dates submerged. These were noted when comparing the archaeological dates of the Oulu region and the dates of Kangas in Kaustinen to the curves by Vuorela et al. (2009: 88, Case1 V2; cf. Paper II: Fig. 7 and Fig. 8; note the entries in Errata). These two discrepancies are not systematic but instead are the result of gaps in the basin isolation records from both regions, in the first case from 800 CE to 4000 BCE and in the second case between 4700–1900 BCE.

If we disregard these two uplift curves where, due to gaps in the basin isolation record, the effect is indicated as too strong to agree with the archaeological record, there remains a considerable possibility that the basin isolation dates are systematically too old. This issue has already been noted in pollen studies (Lahtinen et al. 2016). Thus, error factors in radiocarbon dated gyttja should definitely be investigated further. Systematic errors in chronologies obtained from lakebed core samples could be resolved through calibration (see Lahtinen et al. 2016), which might make basin isolation dates more applicable in future studies of land uplift.

9.4 Critique of recent processualist interpretations

Since high resolution climate reconstruction is in its early stages, it is questionable whether short-term temperature variations should yet be taken into account in archaeological research (Chapter 4.1). Recently, multiple chronological correlations between the past climate and changes in the archaeological record have been brought to attention (e.g. Manninen, 2014; Tallavaara, 2015; Jørgensen, 2020).

Such correlations have, in my experience, almost invariably emphasized environment over culture, with cultural processes, i.e. things that define identities, relegated to the role of species-scale adaptive measures.

Such processualist interpretations frequently invite what sociologist Bruno Latour has described as double-click thinking: a jump from a preliminary analysis straight to a foregone conclusion, without a critical analysis of the epistemological process. Archaeologist Mikael Manninen (2014) argued, without really exploring alternatives, that changing habits in stone knapping were instigated by a cooling climate, because there might be a temporal correlation, give or take a few centuries. Conversely Jørgensen (2020) associates the increased use of polished stone to a warm period, again due to a perceived correlation. Tallavaara (e.g. 2015) studied prehistoric human population density via an assemblage of stacked radiocarbon dates, finding a correlation with reconstructed climate patterns. In such studies, the theoretical connection between the data and conclusion, and also the persuasive power of the argument, lies in the correlation.

Let us take the methodology of inferring the prehistoric population density from radiocarbon date quantities. In Finland, the number of these studies has exploded in the 21st century, although they are mostly the work of a single researcher, archaeologist Miikka Tallavaara, who tends to be the lead author in these studies (Tallavaara et al., 2010; Oinonen, Pesonen, & Tallavaara, 2010; Tallavaara & Seppä, 2012; Tallavaara, Manninen, Pesonen, & Hertell, 2014a; Tallavaara, Pesonen, Oinonen, & Seppä, 2014b; Tallavaara, Luoto, Korhonen, Järvinen, & Seppä, 2015, Tallavaara, 2015; Tallavaara & Pesonen, 2018). As such, the critique seems unfortunately but unavoidably personal. This is not my intent, but merely the circumstances. I deeply respect Tallavaara, who is, for one, a prolific author with the courage to engage a complex issue. His knowledge of prehistoric stone working vastly surpasses mine. I have no doubt that he will consider the critique duly as a scientist and I hope his future research will be better for it. Should my critique be judged as invalid, or disrespectful, I will gladly accept the faults in my reasoning.

The main finding of Tallavaara's research is that early in the Neolithic human population grew rapidly and shrunk at the end of the Neolithic. The studies designate the finding as a 'boom and bust cycle', a term adopted from modern market economics. The 'boom' of the cycle coincides with the Holocene Thermal Maximum. The results were inferred by taking all the radiocarbon dates from archaeological contexts in Finland and looking at how many dates point to which centuries, with some statistical corrections. Periods with many dates equal more

people and correspondingly few dates equal less people. A recent study concluded ‘a dramatic population crash (76% within 200 years)’ at the end of the Neolithic on the southern coast of the Bothnian Arc (Tallavaara & Pesonen, 2018). According to Tallavaara, this population crash, and the fact that the number of radiocarbon dates more or less steadily increases from the Bronze Age onwards, indicates that those who subsisted on foraging were actually more vulnerable to climate change than those practicing agriculture.

Now, I tend to agree with archaeologist Teemu Mökkönen who criticized the methodology of Tallavaara. Why? For several reasons, already laid out by Mökkönen (2014, and dismissed by Tallavaara et al., 2014b). First, during the Neolithic the visibility of archaeological remnants in Finland experienced its own “boom and bust”. Semi-subterranean dwellings began to be used in overwhelming numbers in the early 4th millennium BCE and their use practically ended by the 2nd millennium BCE, coinciding with the population interpretation. Not only this, but Neolithic pottery is much easier to identify than later Bronze Age or Iron Age pottery, which tends to be more fragmented and coarser than the former. Larger polished stone tools, mostly out of use after the Late Neolithic, are much easier to identify should archaeological material appear in sand pits, roadcuts, or during ditch digging, which represent some of the most common ways for the discovery of sites that are not otherwise visible on the surface. There are no statistics of how many such sites have been discovered by the public. Systematic regional archaeological surveys began to be implemented in earnest only after the 1980s, and I argue that prior to this most archaeological sites were discovered by the public, who are particularly adept at identifying Stone Age materiality.

Second, the radiocarbon database was not produced randomly. Many large-scale dating projects have focused on the Stone Age. During the 1990s at least three major dating projects were undertaken, one concerning Neolithic dwelling depressions (see Pesonen, 2002), another focusing on the early habitation in Northern Finland (see Carpelan, 2004a), and a third on Neolithic Comb-Ware Ceramics. Other projects too tend to focus on specific timeframes, often studying periods which are already well known. I do not know of a single project that has focused on enhancing the chronology of the region’s Late Neolithic–Bronze Age transition. The dating of archaeological sites has never been based on random sampling, but more on research interests.

Third, and most importantly, a theoretical basis for correlating radiocarbon date quantities to population levels does not seem to exist. Perhaps the fault is on me for not understanding it. The theoretical basis seems to be that, first, more waste equals

more people; second, a database of radiocarbon datings equals past waste (see Tallavaara, 2015: 17–21). What is not considered is that radiocarbon dates are actually waste picked and chosen by archaeologists. Only a tiny fragment of all dateable material is in fact dated. Thus, in this case, the theoretical link between the data and the interpretation is at best disconnected.

I referred to the dismissal of Mökkönen's critique without elaborating further, so let us dwell on that. Tallavaara et al. in their response presented eight corroborating proxies supporting their hypothesis (Tallavaara et al., 2014b: Fig. 1). The first five proxies are affected by the same biases: high visibility of the Neolithic archaeological record, and research interests, which we will return to later. The sixth proxy shows an Early Neolithic peak in the percentage of seal bones out of all the identified animal bones excavated from coastal sites. The graph offers a compelling argument, but as the data derives from an unpublished database (see Tallavaara, 2015: 30–31), it cannot be independently verified. Publishing the analysis and the used dataset would be highly appropriate, as well as beneficial for wider research, so I hope this will be achieved in the near future.

The dubious seventh proxy relates to Lewis Binford's hunter-gatherer database, representing an overall sketch of observed forager population densities in relation to average temperature (Tallavaara & Seppä, 2012: Fig. 3). The resulting graph seems to be a renumbered replica of the annual mean temperature graph, but with an obscure composition process, so I will refrain from commenting on it in this instance. Even Tallavaara and Seppä (2012) refer to the 'explanatory power of such a simple model' as 'rather weak,' arguing that 'this is not a major concern in our case because we are not interested in predicting actual population densities.'

The eighth proxy is the dating of a genetic bottleneck by Sajantila et al. (1996) coinciding with a decline in other proxies. This study, according to Tallavaara et al. (2014b), is independent from the archaeological record. Another near-independent proxy, the ninth, is the assemblage of radiocarbon samples dated prior to the 1980s, supposedly before research interests had any role in the composition of the radiocarbon assemblage.

Let's take the genetic bottleneck first. The study (Sajantila et al., 1996) initially dated the perceived bottleneck to 25,000 or 10,000 years ago. But the chronology was readjusted based on 'a recent preliminary report', according to which mitochondrial mutations happen faster than was previously thought (1 per 50 generations instead of 1 per 650 or 250 generations), and the bottleneck was set to 3,900 years ago. Next, the study correlates the date with the archaeological record of Finland, noting that ten to five centuries previously the Battle Axe Culture spread

to Finland, heralding the beginning of agriculture, which correlated with the bottleneck (see Sajantila et al., 1996).

Now, the 'recent preliminary report' that Sajantila et al. (1996) cited was a genetic study of the Romanov family of Russia (Ivanov et al., 1996), which in turn cited an unpublished study by the US Armed Forces DNA Identification Laboratory. But the eventually published version of the Armed Forces study actually noted that should the mutation rate be consistently as fast as they found, up to 1 per 30, the human race would only be approximately 6,500 years old (Parson et al., 1997). Thus the lead author concluded, based on inconsistency with the fossil record, that the genetic clock has accelerated, with 'a faster "apparent rate" in more recent times than in the distant past, i.e., the apparent rate is not constant in time' (Armed Forces Institute of Pathology, 1998: 9).

Why Sajantila et al. (1996) decided to calibrate their genetic clock based on unconfirmed preliminary findings is not known, but it would not be surprising that the value (1 per 50) was simply a good fit for connecting the genetic bottleneck to the archaeological proxy. The subsequent repurposing of the genetic bottleneck dating as a proxy in relation to the archaeological record is simply completing the circle. More recent genetic studies have based their bottleneck chronology on Tallavaara's population proxies (see e.g. Sundell, Heger, Kammonen, & Onkamo, 2010). So the wheel keeps on turning.

But more importantly, Tallavaara states, 'the pattern in the temporal distribution of 14 C dates has remained the same throughout the history of radiocarbon dating in Finland. The pattern was the same in the 1970s and 1980s as it is today. It is also the same in Siiriäinen's (1981) curve, which is based on even older data gathered before 1969. Thus, the boom in the research of [Neolithic] house pit sites in the 1990s did not have any profound influence on the shape of the distributions of archaeological material and 14 C dates' (Tallavaara, 2015: 53–54).

Now, this could make for a convincing argument, if not for two things. First, the other way to look at this is that prior to 1980 the visibility of the Neolithic material record was already a factor. For example, the *Muinaiskalupäiväkirja* (Eng. Artefact Diary), maintained by the FHA contains 8,985 entries of archaeological finds from Finland registered in the 19th century, when artefacts were discovered almost exclusively by the public. It is unknown how many of the sites known today were initially found by the public, but my experience, gained from ploughing through certainly more than 500 field reports, suggests that a significant percentage of sites which have been excavated were initially inspected due to knowledge gained from local informants.

Of the diary entries 4,799 are registered as Stone Age, of which the vast majority are easily identifiable polished stone tools. What we now know is that such tools became increasingly rare after the Neolithic, while quartz flakes and small quartz tools act as the main indicators of Stone Age, Bronze Age, as well as Early Iron Age activity until ca. 300 CE. The rest of the diary up to December 31st, 1899 consists of 2,929 historical entries, 875 Iron Age entries, and only 36 Bronze Age entries. This shows that a bias toward the Stone Age, especially in the form of polished—again, mostly Neolithic—stone tools, exists at the very foundation of Finnish archaeology. Early on, archaeology students have to be specifically taught to identify and keep an eye out for quartz flakes, meaning that even today the public does not by default possess the knowhow to positively identify the main, and arguably less biased, prehistoric archaeological indicator.

Second, the number of dates involved prior to 1990s is nowhere near a representative sample. The same OASIS-database used by Tallavaara et al. (2014b: Fig. 2; also Tallavaara, 2015: 45–46) in their counterargument indicates that in 1980 the record of 4000–6000 BP, the "boom-and-bust" range, consisted of only 41 radiocarbon dates from 24 different sites. Hardly statistically valid, these numbers make it possible, in this short instance, to study the contexts of the dates one by one.

Of the 41 samples, 13 were apparently submitted by C. F. Meinander in preparation for the publication 'Radiokarbondateringar till Finlands stenålder' (1971). Just omitting these clearly selected non-random samples would seriously shrink the slight uptick of dates within the boom-and-bust range. Not only that, but upon further examination, the reports of the excavations themselves indicate that the contexts of ten samples were already known to be Neolithic because the sites contained already identified Neolithic pottery, red-ochre burials, or both. A further 11 dates came from sites that in their field reports were strongly suspected to be Stone Age sites. Only seven times out of 41, may the archaeologists choosing the dating samples have been unaware of the likely results. This was supposedly before any considerable bias resulting from research interests (see Tallavaara, 2015: 54).

It can be argued that Tallavaara's response, that the opposing 'argument is invalid, and will remain so until it is shown there is indeed such a strong correlation between the current visibility of archaeological remains and the frequency of 14 C dates (or known sites) in Finland' (Tallavaara, 2015: 54), is seriously undermined by the previous examination. To suppose that the individual research interests of archaeologists has not had an overwhelming bias on the data that they have not only collected but consumed time and effort studying is simply wishful thinking.

Also, to suppose that the different levels of visibility within the archaeological record has not affected research interests, and the public's ability to identify and inform archaeologists of new sites, defies logic.

Such assumptions are in danger of confusing disassociated actions as correlates of studied phenomena. At this point it is my opinion that a caveat should accompany the studies which lean on Tallavaara et al. (2010) and subsequent publications of the same line of inquiry. The caveat should be reassessed if the authors study in earnest the formation history of the archaeological record of Finland with a critic's eye. They should prove that biased research orientation and public's attentiveness to Finland's Neolithic has not significantly skewed both the radiocarbon date record and the quantities of recovered materiality, instead of shifting the burden of proof onto others.

One thing remains unsolved, even if we were to accept the archaeological record as an unbiased random sample. Why would people who subsisted on foraging be so vulnerable to climate cooling at the end of the Neolithic? Were they not capable of adapting to new circumstances, even when Fennoscandia, while for the most part relatively inhospitable to agriculture, maintained a rich biosphere even during winter months? According to some studies, the winter months were indeed more affected by cooling events than summer months. Did the woodland, marine, and freshwater fauna, and even the flora, which on the ground level is partly preserved by frost and snow cover, diminish in such significant measures to cause starvation for the foragers, resulting in one of the worst human catastrophes in the region's past, with a reduction of population by up to "76%"? Should we assume that early underdeveloped agriculture proved more resilient to climate change than foraging with millennia of cumulative experience? Or should we acknowledge that the biases of the archaeological record have inflated the footprint of the Neolithic and accept that the following *absence of evidence is not evidence of absence*.

Whether such processualist accounts, *where correlation equals causality*, are accepted despite criticism remains to be seen. I certainly have no reason to dismiss them altogether. It is more than reasonable to believe that the changing climate affected people in many ways. It may have even changed their whole worldview, and perhaps, due to cultural adaptation or even actual famine, population levels. There is no denying the possibility.

The worst recorded famine in Finland, the Great Famine of 1695–1697 CE, caused in large part by short-term climate deterioration, killed approximately a third of the population of the agricultural regions. Its effect on foragers, mainly relegated to the Arctic regions, is difficult to assess, although historian Mirkka

Lappalainen noted a census entry from 1697 CE concerning the foragers of Lapland, the Woodland Sámi. According to the entry, 'if the Lapps do not catch fish or deer soon, they will die of starvation' (Lappalainen, 2012: 111). This could be taken as an indication that even a foraging subsistence was highly vulnerable to cooling events.

But overfishing on the lower runs of the large rivers of the Bothnian Bay had already become a major factor during the preceding 16th and early 17th century CE, aggravated by mercantilism and taxation (Virrankoski, 1985: 215–216; Enbuske, 1996: 328–345). Also, court records a decade prior to the Great Famine reveal that the Woodland Sámi made frequent complaints about slash-and-burn farming expanding into their hunting grounds, driving away the local fauna (Virrankoski, 1985: 209). Furthermore, the European fur trade had significantly reduced small mammal populations (Enbuske, 1996: 345). So at this stage, the ideology of production already inhibited the procuring way of life, arguable more than climate cooling. In previous instances of abrupt climate cooling, when mainly foragers occupied the region and there is no indication of fauna being overly encumbered due to extra-subsistence production by humans, were the forager communities more resilient to climate change than later on when animal populations had become diminished by overproduction?

While we cannot completely dismiss the interpretations deduced from the radiocarbon date record, the lack of theory simply makes such interpretations too fragile to accept at face value should one possesses a critical outlook. Yet, more and more keep being published on top of the rickety foundations. What I urge is that such studies should adhere to persuading their readers not so much with technical jargon, but with a clear and contemplative theoretical framework, which secures the link between the material and the interpretation. Again, with such a critique I do not wish to insult anyone, and I apologize if I do. I merely hope that my commentary enhances the persuasiveness of archaeological research in all specializations by understanding that the whole field, as well as natural sciences and statistics, is grounded in philosophy.

9.5 Facing the mirror

> For such is the nature of men, that howsoever they may acknowledge many others to be more witty, or more eloquent, or more learned ; Yet they will hardly believe there be many so wise as themselves : For they see their own wit at hand, and other men's at a distance. (Hobbes, 1651/1909: 94–95)

After criticizing others, it is only fair to subject my own work to at least some scrutiny. As Thomas Hobbes surmised, I do not expect to be a neutral observer when evaluating my own output. It should be acknowledged that ambiguity in a research article is often analogous to shooting oneself in the foot, and sometimes expressions of uncertainty result in an unpublished paper, whereas complete certainty is much more publishable. The first thing I would like to change in all my papers is adding maybes, perhapses, ors, and possiblys, and after that doubling them.

I would summarize my own self-subjected critique in the following ways. First, the social interpretations presented in Paper I are mostly at odds with my current thinking (see Chapter 7.2). This is actually rather fortunate since it serves to highlight the influence of the interpretational key in defining the outcome of archaeological studies. When writing Paper I, theory was not the first thing on my mind, and, in my opinion, it shows. During the 7-year process, however, the foothold of theory strengthened considerably in my thinking. Thus, Paper I written today would be unrecognizable.

Secondly, the study of the shoreline displacement chronology in Paper II comes uncomfortably close to a similar kind of circular thinking that I have accused others of. I used archaeological sites as benchmarks to establish the uplift curves, and then reflected the chronology back onto archaeological sites. The veracity hangs on a single thread, the relative chronology of the four radiocarbon dates at Purkajasuo indicating the contemporary shoreline. Should this thread snap, especially the chronology of Paper V becomes suspect. Thus, more absolutely dated archaeological benchmarks signifying the contemporary shoreline are desperately needed from different regions.

Third, in Paper III I continued using a group categorization that remains too similar to the concept of society. Dividing the two observed prehistoric contexts into subsistence ideological cultures is undoubtedly a rough binary division, which may well be more skewed than reflective of lived reality. If instead of a binary division the record was studied with smaller group categories, such as regional division based on living geography (see Chapter 4.2), I suspect the results would differ. Yet, the analysis in Paper III is meant as the highest-level perspective that in my mind has any pertinence, and it is followed up with the more focused perspective of Paper V in order to supplement the abstractness of the earlier study.

The fourth issue is that my own recent experiences in confronting death undoubtedly affected the choice of interpretational key in Paper IV. Some may find this a dubious method to guide analysis, but I would argue that a little empiricism

never hurt anyone. And fifth, the analysis of Paper V and the supplemental analysis in Chapter 7.1 is heavily dependent on a chronology and archaeological record, which are both filled with gaps. Also, and more importantly, the analysis was partly formulated in a way that cannot fail. If the goal of a binary juxtaposition is to reveal differences, it will undoubtedly succeed in doing so at some level. I hope that the study is understood only as a preliminary step in trying to understand local differences that in all likelihood were more pronounced during prehistory than we currently envision.

The question remains, overall, whether my interpretations are more objective than subjective. I guess this is for the reader to decide. I argue that subjectiveness is not necessarily unscientific, as long as it is not masquerading as objectiveness. Thus, the concept of the interpretational key is extremely useful in unmasking pre-existing assumptions.

On the Discovery Channel's series *Manhunt,* the issue of the interpretational key was summarized rather effectively in a scene with two FBI agents arguing over the psychological profile of the Unabomber. Seemingly proving the validity of his chosen psychological profile, a disgruntled airline mechanic, the higher-ranking agent presents as evidence a photo of a bomb switch resembling the tail stand of a jet airliner. To this the lower ranking agent responds: 'That only looks like a tail stand if you're looking for proof of the supposition that he's an airline mechanic. If you're objective about it, it's just a switch.' It is up to the reader to decide whether the interpretations I have presented here are more objective than not. Also, whether in the face of new data and methodology my interpretations stand the test of time, only time will tell.

10 Conclusions

In conducting this study I have ultimately tried to answer this question: what is a prehistoric society and how does it apply to the past in Central Fennoscandia? My conclusion, deriving from theoretical scrutiny and material analysis, is that society is an abstraction, a linguistic cheat for the archaeologist to use when group organization eludes definition. The argument can also be applied to the concept of prehistoric economy, which has been used variously to describe household subsistence, transactions without currency, and anachronistic projection of the metaphysical marketplace.

Whether I have managed to adequately implement the criticism in the context of my own research is questionable. Still, what I argue is that, instead of using such abstractions to bypass difficult questions, we should focus on the more difficult task of understanding prehistory as pluralistic and multivalent, and rooted not in systems but in individuals. I do not mean this as a dismissal of the value of studying systems, but I do mean that metaphysical systems invariably grow outward from the local. Systems such as culture, history, tradition etc. undoubtedly affect individuals in return. Yet, if the local perspective is disregarded, any system is in danger of being found to be a mere fusion of unrelated correlations.

In the archaeological context, among the most grounded foci for studying the local are communities. Community is represented in many forms in the archaeological record. Along the Bothnian Arc and in Central Fennoscandia, where state organization has emerged only within the last five centuries, societies of the preceding times should not be contextualized as homogenized mass. Instead, I argue, we should pursue more systematic investigations of local identities. The same issue applies throughout the world, in archaeology and elsewhere, even though the current ideal of globalization tends to push research to the other extreme.

Despite this globalizing trend, it might be worthwhile to consider local communities seriously as a backbone of everyday life. I hope that in conducting this research project, I have managed to make the case for a slight readjustment of archaeological research in order to shed systematic light, in time, on the multitude of local communities, their neighbors, and the worlds around them. The kinds of systems that such analyses could reveal are yet to be discovered.

In this synthesis I have brought up several uncertainties and assumptions regarding population density as well as prehistoric mobility, and also the radiocarbon dating of gyttja samples, where the underlying methodological foundations are precarious at best. Any faults in these foundations have significant

consequences for the interpretations deriving from them. I suggest revisiting the issues, with critical analysis and open-minded discussion, to assess whether they skew our understanding of past communities, environments, and lived experiences. In doing so, the cohesiveness of the methodological—especially theoretical—chains of sequence may either be re-established or found lacking or detached. Future efforts to ensure the veracity of the epistemological lineages of these studies would not go amiss.

What I have not attempted is to define how exactly local communities should be defined in the archaeological record. This is affected by too many variables differing in time and space, such as the noted issues of population density and mobility, which in the context of Central Fennoscandia remain unresolved. I have used the concept of living geography as a delimiter, but this only works in specific cultural contexts. More generalized definitions for the concept of local community requires further research.

The three main perspectives of the analyses performed here derive from different interpretational keys. Based on the theoretical toolkit assembled for the study and the analyses performed within its framework, the prehistoric communities of the Bothnian Arc were cosmologically rooted in foraging existence through the ideology of procuring. Procurement ideology means trusting the surrounding living world to provide livelihood through interaction. While these communities may have even adopted different methods of cultivation and animal tending at different times, their worldview during prehistory did not seem to have ultimately changed to one where hegemony over flora and fauna was morally appropriate. This helps to explain the materialistic differences in Fennoscandian prehistory between the north and south, which are commonly understood as foraging and farming contexts.

Beginning from the Middle Neolithic, in the southern region surrounding the Bothnian Sea and the Kvarken, the ideology of production manifested itself not only in subsistence, i.e. herding and later farming, but also in the material remnants that were not strictly related to subsistence activities. Hierarchical control over nature may have justified hierarchies within and between the local communities. During the Bronze Age human hierarchies seem to have strengthened, as indicated by large burial monuments which required the organization of extra-subsistence labor. In certain southern regions of Iron Age Central Fennoscandia, remnants of metal working, monumental burial mounds, and hill forts reflect maintained political hierarchies and intense extra-subsistence labor.

Meanwhile in the Mid-Iron Age, the archaeological record of the northern procurer zone experienced a brief uptick in its visibility, turning subsequently nearly invisible, and coming back to light in the Late Iron Age after several centuries of apparent absence. While the procurer communities of the Bothnian Arc at times adopted productive activities, they were repeatedly abolished, suggesting that the northern communities chose to periodically revert to political anarchism instead of consolidating hierarchy.

I do not expect that this binary division between two polar opposites is a sufficiently accurate representation. Instead it is an abstraction, based on a perceived divergence within modes of subsistence and labor. The abstraction can rightly be criticized as reflecting homogenized society. Should the same labor analysis be conducted with a multiplicity of communities instead of binary opposites as the framework for group assemblages, the results would offer higher regional resolution. Nevertheless, the binary division may represent two actually divergent worldviews in the region. Understanding the prehistory of Central Fennoscandia and the world around it in this context offers a more plausible representation of lived reality than divisions based on modern borders.

Although the communities of the north and south seem to have differed in their subsistence ideologies, they shared multivalent relationships with death. We should assume that all the communities respected their dead with the same vigor. But traditions regarding the dead were pluralistic, and we cannot be certain that even neighboring communities shared the same mortuary practices. Clear uniformity came relatively recently after the spread of Christianity, which reshaped local mortuary practices and relationships with death, ultimately dismantling lineages of cultural heritage tracing back millennia.

In the final case study, a high-resolution analysis comparing two separate yet culturally related regions within the Bothnian Arc, we saw that the local communities experienced diverged prehistories with their own unique local character throughout the past. I suspect that the divergence is representative of all prehistoric local communities in Central Fennoscandia, although assessing the level of variety requires further research. At different periods, the divergence was more pronounced than in others, which arguably reflects developments taking place throughout Eurasia. Still, the local communities were not simple receptacles of outside influences, but each formulated their own actions, managing dependencies and engaging with the world in the context of undulating networks of action. Continued efforts to trace these actions may offer tangible pathways into a world long gone.

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Errata

Paper II.

- Figure 8 refers the Vuorela et al. (2009) curve to Fig. 51, when it should be Fig. 49 (Kronoby Case1 V2).
- In Appendix 1, the Halosentörmä [3] date GrA-63518 indicates the context as a cooking pit, when it is actually a smaller pit hearth.

Paper IV.

- The reference list contains several editorial errors, especially regarding DOI numbers and URLs (e.g. Núñez, 2015a redirects to an unrelated 2012 paper of the Journal of Hand Surgery, while Núñez, 2015b URL is missing a zero [/2016/**01**/nunez_remedies_against.pdf]; also Valk, 1999 DOI number leads to the wrong study).
- Table 1 apparently lost its original formatting during the editorial process, resulting in false headers. Admittedly, I did not include any headers in the original table, so the mistake is ultimately mine.

Original publications

I Hakonen, A., Hakamäki, V. & Kuusela, J.-M. (2017). Observing social change on the Bothnian Bay coast in the 1st millennium BC: The burials of Tahkokangas and the community of the Oulujoki river estuary. *Assemblage*, 15*,* 15–27. https://assemblagejournal.files.wordpress.com/2017/05/hakonen-et-al-to-submit.pdf

II Hakonen, A. (2017). Shoreline displacement of the Finnish Bothnian Bay coast and the spatial patterns of the coastal archaeological record of 4000 BCE – 500 CE. *Fennoscandia archaeologica*, 34*,* 5–31. http://www.sarks.fi/fa/PDF/FA34_5.pdf

III Hakonen, A. (in press). Interpreting prehistoric labor north and south of the forager-agricultural frontier in Central Fennoscandia, Northern Europe. *Arctic Anthropology*.

IV Hakonen, A. & Hakamäki, V. (2019). Living with death: what moral consideration of mortuary practices reveals about the plurality of worldviews in the multi-millennial past of Central Fennoscandia. *Time & Mind*, 12(4), 287–304. https://doi.org/10.1080/1751696X.2019.1681744

V Hakonen, A. (2021). Communities Beyond Society: Divergence of Local Prehistories on the Bothnian Arc, Northern Europe. *Open Archaeology*, 7(1), 211–230. https://doi.org/10.1515/opar-2020-0132

Original publications are not included in the electronic version of the dissertation.

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